THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY.

"Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

THE PHILOSOPHICAL MAGAZINE.

OCTOBER 1798.

I. *Observations on a Mass of Iron found in Siberia by Professor Pallas, and on other Masses of the like Kind, with some Conjectures respecting their Connection with certain natural Phenomena. By Professor* CHLADNI *of Wittenberg.*

THE mass of iron found in Siberia by Pallas, and described in the third volume of his Travels*, is, in many respects, so like the masses described by Buchholz†, Rubin de Celis‡, Dr. Lober§, Nauwerk‖, and others, that there is great reason to conclude, they have all had a similar origin. This seems also to be in some measure confirmed by a circumstance mentioned by Pallas, who says, that the Tartars considered the mass to which he alludes, as a sacred relick that had dropped from heaven.

The problematic mass in question was found between Krasnojarsk and Abekansk in the high slate mountains, quite open and uncovered. It weighed 1600 pounds; had a very

* Page 311.

† Naturforscher, part iv. page 227.

‡ Philosoph. Transactions, vol. lxxviii. part i. page 37.

§ Berl. Sammlungen, vol. vii. page 523.

‖ Crell's Beytr. zu d. Chem. Annalen, vol. i. part ii. page 86.

irregular and ſomewhat compreſſed figure like a rough granite; was covered externally with a ferruginous kind of cruſt; and the inſide conſiſted of malleable iron, brittle when heated, porous like a large ſea ſponge, and having its interſtices filled with a brittle hard vitrified ſubſtance of an amber yellow colour. This texture and the vitrified ſubſtance appeared uniformly throughout the whole maſs, and without any traces of ſlag or artificial fire.

Dr. Chladni ſhews with a great deal of ingenuity, that this maſs neither originated by the wet method; nor could have been produced by art, the burning of a foreſt, by lightning, or by a volcanic eruption. It appears to him much more probable that it is of the ſame nature as the ſo called fire-balls (*bolides*) or flying dragons, and he quotes a variety of obſervations made on theſe phenomena; from which he endeavours to prove that they do not ariſe from an accumulation of the matter of the aurora borealis; a tranſition of electricity from one part of the atmoſphere to another; an accumulation of porous inflammable ſubſtances in the higher regions, or the catching fire of a long train of inflammable air; but that their component parts muſt be conſiderably denſe and heavy, as their courſe ſhews in ſo apparent a manner the effects of gravity; and becauſe their maſs, though it diſtends to a monſtrous ſize, retains ſufficient conſiſtency and weight to continue an exceedingly rapid movement through a very large ſpace, without being decompoſed or diſſolved, notwithſtanding the reſiſtance of the atmoſphere. It ſeems to him probable, that this ſubſtance is by the effect of fire reduced to a tough fluid condition; becauſe its form appears ſometimes round and ſometimes elongated, and as its extending till it burſts, as well as the burſting itſelf, allows us to ſuppoſe a previous capability of extenſion by elaſtic fluidity. At any rate, it appears to be certain, that ſuch denſe matter at ſo great a height is not collected from particles to be found in our atmoſphere, or can be thrown together into large maſſes by any power with which we are acquainted;

acquainted; that no power with which we are acquainted is able to give to ſuch bodies ſo rapid a projectile force in a direction almoſt parallel to the horizon; that the matter does not riſe upwards from the earth, but exiſts previouſly in the celeſtial regions, and muſt have been conveyed thence to our earth. In the opinion of Dr. Chladni, the following is the only theory of this phenomenon that agrees with all the accounts hitherto given; which is not contrary to nature in any other reſpect; and which beſides ſeems to be confirmed by various maſſes found on the ſpot where they fell.

As earthy, metallic and other particles form the principal component parts of our planets, among which iron is the prevailing part, other planetary bodies may therefore conſiſt of ſimilar, or perhaps the ſame component parts, though combined and modified in a very different manner. There may alſo be denſe matters accumulated in ſmaller maſſes without being in immediate connexion with the larger planetary bodies, diſperſed throughout infinite ſpace, and which, being impelled either by ſome projecting power or attraction, continue to move until they approach the earth or ſome other body; when being overcome by their attractive force, they immediately fall down. By their exceedingly great velocity, ſtill increaſed by the attraction of the earth and the violent friction in the atmoſphere, a ſtrong electricity and heat muſt neceſſarily be excited, by which means they are reduced to a flaming and melted condition, and great quantities of vapour and different kinds of gaſes are thus diſengaged, which diſtend the liquid maſs to a monſtrous ſize, till, by a ſtill farther expanſion of theſe elaſtic fluids, they muſt at length burſt. Mr. Chladni thinks alſo, that the greater part of the ſhooting-ſtars as they are called, are nothing elſe than fire-balls, which differ from the latter only in this, that their peculiarly great velocity carries them paſt the earth at a greater diſtance, ſo that they are not ſo ſtrongly attracted by it as to fall down, and therefore in their

paſſage through the high regions of the atmoſphere, occaſion only a tranſient electric flaſh, or actually take fire for a moment, and are again ſpeedily extinguiſhed, when they get to ſuch a diſtance from the earth that the air becomes too much rarefied for the exiſtence of fire.

The grounds on which Dr. Chladni founds the above theory, reſpecting the origin of the before-mentioned maſs of iron, and its ſimilarity to the productions of fire-balls, are farther explained by the author in the following manner:

1. As fire-balls conſiſt of denſe and heavy ſubſtances, which by their exceedingly quick movement, and the friction thence excited by the atmoſphere, become electric, are reduced to a ſtate of ignition, and melted by the heat, ſo that they extend to a great ſize, and burſt; it thence follows, that in places where fragments, produced by the burſting of a fire-ball, have been found, ſubſtances endowed with all theſe properties muſt alſo have been found. Iron, however, the principal component part of all the maſſes hitherto found, poſſeſſes all theſe properties in a very eminent degree. The weight and toughneſs of the principal component parts of fire-balls, which muſt be very conſiderable, ſince, with the greateſt poſſible diſtenſion, they retain conſiſtence enough to proceed with the utmoſt velocity through ſuch an immenſe ſpace without decompoſition of their maſs, and without their progreſs being obſtructed by the reſiſtance of the air, agree perfectly well with melted iron; their dazzling white light has by many obſervers been compared to that of melted iron; iron alſo exhibits the ſame appearances of flaming, ſmoking, and throwing out ſparks, and all theſe phenomena are moſt beautiful when they take place in vital air. Of the extenſion by elaſtic fluids expanded by the heat, and of the contraction which follows from cold, traces may be diſcovered in the internal ſpongy nature of the iron maſſes which have been found, and in the globular depreſſions of the exterior hard cruſt, the latter of which gives us reaſon to ſuppoſe, that in theſe places there have been air-bubbles,

which,

which, on cooling, ſunk down. The mixture of ſulphur found in various maſſes, agrees alſo exceedingly well with the phenomena of fire-balls, and eſpecially with the great inflammability of ſulphur in very thin impure air; for it is well known that ſulphur in an air-pump will take fire in air in which few other bodies could do the ſame. In regard to thoſe maſſes in which no ſulphur was found, this may have ariſen from the ſulphur eſcaping in vapour, ſince ſome time after the appearance of fire-balls a ſtrong ſmell of ſulphur has been perceived. The brittleneſs of the Siberian iron maſs when heated, may ariſe from ſome ſmall remains of ſulphur, which may perhaps be the cauſe of the facility with which fragments of this maſs, as well as that found at Aix-la-Chapelle, could be roaſted.

2. The whole texture of the maſſes betrayed evident ſigns of fuſion. This, however, cannot have been occaſioned by any common natural or artificial fire; and particularly for this reaſon, becauſe iron ſo malleable is not fuſible in ſuch fire, and, when it is fuſed with the addition of inflammable matters, loſes its malleability, and becomes like common raw iron. The vitrified ſubſtance in the Siberian maſs is equally incapable of being fuſed in a common fire. The fire then muſt have been much ſtronger than that produced by the common natural and artificial means, or the fuſion muſt have been effected by the force of exceedingly ſtrong electricity, or perhaps both cauſes may have been combined together.

3. Beſides the ſimilarity of the above-mentioned maſſes, the circumſtances reſpecting their falling are ſo like, that they cannot be conſidered as merely accidental, and therefore give more credibility to the accounts. This being premiſed, the principal queſtion then is, whether the phenomena of this kind obſerved were the effects of thunder, or aroſe from detached fragments of fire-balls? Several circumſtances quoted by Dr. Chladni ſeem to oppoſe the idea of their ariſing from thunder; but agree ſo perfectly with

every circumſtance remarked in regard to fire-balls, that, without aſſerting any thing abſurd, every relation of this kind may, he ſays, be admitted in their full and literal extent. In the atteſtations reſpecting that of Agram, it is mentioned that ſeveral people, in different parts of the kingdom, ſaw and obſerved the burſting of the fire-ball, and heard its exploſions and noiſe in the atmoſphere, and perceived alſo that ſomething fiery fell from the heavens, ſo that it was a circumſtance generally known in the neighbourhood, only that on account of the diſtance it was not exactly known in what place it had fallen. Hence it may be concluded, that it was no effects of lightning, but an actual fire-ball. Lightning, at a time of the year when ſtorms are not uncommon, would not have excited univerſal attention, much leſs would it have been obſerved at a great diſtance as a falling fiery maſs. From the ſimilarity of the maſſes, it is alſo highly improbable that, in all the places where they were found, fuſible parts only ſhould have been diſcovered, and ſhould have been changed in the ſame manner by lightning; eſpecially as in places ſtruck by lightning no maſſes of the like kind have been found, but always vitrified earthy particles, &c.

4. It is totally incomprehenſible how, on the high ſlate mountains, where the Siberian maſs was found, at a conſiderable diſtance from the iron mines; in the chalky ſoil of the extenſive plains of America, where for a hundred miles around there are no iron mines, and not even ſo much as a ſtone to be found; and at Aix-la-chapelle, where, as far as the author knows, there are no iron works, ſo many ferruginous particles could be collected in a ſmall ſpace, as would be neceſſary to form maſſes of 1600, 15000, and 17000, up to 33600 pounds. This circumſtance ſhews that theſe maſſes could as little have been fuſed by lightning, as by the burning of a foreſt, or of foſſile coal. Theſe maſſes were found quite expoſed and uncovered, and not at any depth in the earth, where we can much more readily admit

ſuch

ſuch an accumulation of ferruginous particles to have been melted by the effects of lightning.

Should it be aſked how ſuch maſſes originated, or by what means they were brought into ſuch an inſulated poſition; this queſtion would be the ſame as if it were aſked how the planets originated. Whatever hypotheſes we may form, we muſt either admit that the planets, if we except the many revolutions which they may have undergone, either on or near their ſurface, have always been ſince their firſt formation, and ever will be the ſame; or that nature, acting on created matter, poſſeſſes the power to produce worlds and whole ſyſtems, to deſtroy them, and from their materials to form new ones. For the latter opinion there are, indeed, more grounds than for the former, as alternations of deſtruction and creation are exhibited by all organiſed and unorganiſed bodies on our earth; which gives us reaſon to ſuſpect that nature, to which greatneſs and ſmallneſs, conſidered in general, are merely relative terms, can produce more effects of the ſame kind on a larger ſcale. But many variations have been obſerved on diſtant bodies, which, in ſome meaſure, render the laſt opinion probable. For example, the appearing and total diſappearing of certain ſtars, when they do not depend upon periodical changes. If we now admit that planetary bodies have ſtarted into exiſtence, we cannot ſuppoſe that ſuch an event can have otherwiſe taken place, than by conjecturing that either particles of matter, which were before diſperſed throughout infinite ſpace, in a more ſoft and chaotic condition, have united together in large maſſes, by the power of attraction; or that new planetary bodies have been formed from the fragments of much larger ones that have been broken to pieces, either perhaps by ſome external ſhock, or by an internal exploſion. Let whichever of theſe hypotheſes be the trueſt, it is not improbable, or at leaſt not contrary to nature, if we ſuppoſe that a large quantity of ſuch material particles, either on account of their too great diſtance, or becauſe prevented by

a ſtronger movement in another direction, may not have united themſelves to the larger accumulating maſs of a new world; but have remained inſulated, and, impelled by ſome ſhock, have continued their courſe through infinite ſpace, until they approached ſo near to ſome planet as to be within the ſphere of its attraction, and then by falling down to occaſion the phenomena before mentioned.

It is worthy of remark, that iron is the principal component part of all the maſſes of this kind hither to diſcovered; that it is found almoſt every where on the ſurface of the earth, as a component part of many ſubſtances in the vegetable and animal kingdom; and that the effects of magnetiſm give us reaſon to conclude, that there is a large proviſion of it in the interior parts of the earth. We may therefore conjecture that iron, in general, is the principal matter employed in the formation of new planetary bodies; as is ſtill farther probable by this circumſtance, that it is excluſively connected with the magnetic power*; and alſo on account of their polarity, may be neceſſary to theſe bodies. It is alſo probable, if the above theory be juſt, that other ſubſtances contained in ſuch fallen maſſes, ſuch as ſulphur, ſiliceous earth, manganeſe, &c. may be peculiar not to our globe alone, but may belong to the common materials employed in the formation of all planetary worlds.

* The Profeſſor ſeems not to have been acquainted with a fact announced in our firſt volume (p. 426), that ſeveral other ſubſtances beſides iron poſſeſs magnetic properties. EDIT.

II. *Strictures*

II. *Strictures on Mr. Joseph Collier's "Observations on Iron and Steel*." By Mr. David Mushet, of the Clyde Iron Works. Communicated by the Author.*

It is much to be wished, that men practically versed in the various manufactures of Britain would turn their attention to the best means of disseminating a knowledge of the principles and operations which have been determined by experience as the best to be followed in the large way, according to local and other circumstances. A candid and liberal communication of individual observation, by promoting the common interest, would tend ultimately to the benefit of each manufacturer, by the increased improvement and perfection of their various articles; for the real welfare of any particular branch depends less upon the superiority of one man's article over that of another, in the same line, than upon the general superiority of a national product over that of any other country—a pre-eminence that depends entirely on the aggregate mass of industry, ingenuity and intellect exerted in the one or the other.

What I recommend is the more necessary, as inaccurate and fallacious principles are often brought forward by men of science, even the best intentioned, from a want of that practical knowledge, which can only be acquired by a long and personal acquaintance with the processes carried on in the large way of manufacture. The mischiefs hence occasioned are incredible: it tends to separate the man of science and the manufacturer; it shackles the latter with increasing prejudice; makes him view the former with a suspicious eye; is the principal reason why science has been so long excluded from our manufactories; and why the accurate results of the laboratory have so long been despised by the practical artist, and been deemed undeserving of experi-

* See Phil. Mag. vol. i. p. 46.

ment

ment on an extended ſcale. The artiſt and the man of ſcience ſhould mutually inform each other: principles will then, and not till then, acquire conſiſtence and correctneſs, and their value will be eſtabliſhed on the ſureſt foundation.

The precept I inculcate I ſhall myſelf follow, to the beſt of my ability, by communicating, through the medium of the Philoſophical Magazine, a ſeries of papers intended as an introduction to a knowledge of the manufacturing of iron in this country, with details of the various proceſſes in uſe: to be followed by a ſeries of experiments relative to the principles of iron and ſteel, which have occupied my leiſure time for ſome years paſt, and which preſent ſome facts not generally known. But as it may tend to clear the path, and enable me afterwards to proceed with leſs interruption, I ſhall in this paper confine myſelf chiefly to the correcting of ſome miſtakes into which Mr. Collier, in his Obſervations on Iron and Steel, has been betrayed by a want of knowledge in the mechanical proceſſes now in uſe, and the minute practical details of the various modifications of iron. Theſe can only be deduced, with certainty, from a keen, perſonal, and long continued inveſtigation of the confirmed modes of practice on a large ſcale. To ſpread information on theſe intereſting ſubjects, and to correct erroneous opinions, formed from the peruſal of writers eminent in the ſcientific walks of life, are the reaſons ſtated by Mr. Collier for preſenting the public with his Obſervations. To paſs over his obſervations in ſilence, would be doing him as well as the public injuſtice, ſince it would be to aid the diſſemination of that error, which he ſo laudably wiſhes to root out; in his endeavours to the performance of which, he ſurely ought to meet with the aſſiſtance and approbation of every one intereſted for the welfare and proſperity of his country.

One rock on which Mr. Collier ſplits, is in cenſuring and quoting the French chemiſts, and others, without recollecting whether the fuel uſed is the ſame in France as in Britain;

Britain; and alſo that chemical writers in general, from neceſſity as often as choice, confine their experiments ſimply to the laboratory: theſe, however accurate, will always be found materially varied when followed out on a large ſcale.

When M. Fourcroy ſtates the length of time neceſſary for cementing ſteel, moſt evidently he alludes, not to ſteel manufactured in the large way, but to that effected in chemical experiments; elſe, he never could have mentioned an *earthen box*. It is well known that bars of iron $\frac{1}{3}$th of an inch in thickneſs, the length is no way material, will paſs into the ſtate of good ſteel during 12 hours cementation with a white heat. Had M. Fourcroy meant any other than *bar ſteel*, he would not have mentioned iron as the ſubſtance to be operated upon.

In regard to the time neceſſary for cementing ſteel on a large ſcale, Mr. C. has ſtruck the extreme from the chemical gentlemen whom he quotes, and who only give the reſults from their laboratories. In the lengthened proceſs of 16 days, ſtated by Mr. C. as being the requiſite time for cementing malleable iron into ſteel, he includes the time which the furnace is allowed to cool, before the workmen can penetrate to the contents; and this period may be lengthened or ſhortened from local circumſtances. The proceſs, I preſume, is reckoned complete when the iron is deemed ſufficiently ſaturated with carbon: this is judged of by the trial bars: if theſe are found perfect, then the whole converſion will be in a ſimilar ſtate: ſhould the furnace after this remain a month unemptied, this adds nothing to the real length of time neceſſary for cementation: I have ſeen ſeveral inſtances, where the workmen have emptied the furnace of high converted ſteel, for drawing into German ſteel, upon the 11th day.

It would be a very difficult matter for Mr. Collier to point out any greater advantage which ſteel poſſeſſes, than that noticed by Mr. Nicholſon; namely, its property of becom-

ing

ing hard when plunged into cold water. This alone gives it an almoſt entire maſtery over all other metals and ſubſtances; it conſtitutes its ſolidity, durability, and poliſh; in ſhort, all its utility, when manufactured into the many various inſtruments of uſe, is derived from this unrivalled peculiarity of the metal.

It is impoſſible to diſcover what could have led Mr. C. in attempting an explanation of the uſe of "plunging into water," to ſay, that it is done to prevent the ſteel "returning to the ſtate of malleable iron:" the fallacy of this aſſertion muſt be obvious to the leaſt intelligent workman. Plunging and heating alternately are the ſpeedieſt methods for bringing ſteel back to malleability: the ſeldomer it is plunged or heated, the longer it will preſerve the carbonic principle, and conſequently remain good. Has Mr. C. been told that at ſteel manufactories, where bar ſteel is drawn into ſhapes, and bars of various dimenſions, that all theſe are immerſed in water, leſt they ſhould, if left without this precaution to cool in the open air, become malleable iron? Were this the caſe, ſteel could never be ſubjected to the file—nor could files be ſtruck—becauſe ſteel could not then exiſt unleſs in a hardened ſtate. Let me aſk Mr. Collier, whether, at thoſe manufactories of ſteel which he has frequented, the trial bars taken from the aperture of the furnace were found, when cold, in the ſtate of malleable iron, or otherwiſe?

The note at the bottom of page 47 is extremely vague. Can there be an expreſſion more indefinite, than that "a partial abſtraction of oxygen takes place by plunging hot metal into cold water?" From what is the oxygen abſtracted? Is it from the hot metal, or is it the oxygen of the decompoſed water? If from the former, pray what ſtate of the metal does Mr. C. allude to? Pure ſteel contains no oxygen; malleable iron, properly manufactured, is alſo void of the baſe of vital air: crude iron alone contains oxygen, an additional quantity of which it abſorbs, when *dropt fluid* into

into cold water. I ſhall therefore ſuppoſe he meant to ſay, that the oxygen of the decompoſed water partially united with the hot metal: this, without further explanation, would be ſtill very unſatisfactory; ſince it does not ſpecify the modification of the metal operated upon. Plunge very hot malleable iron into cold water, and part of the water will be decompoſed; its oxygen will unite with a portion of the iron; this portion, however, will be found to have ſeparated from the bar or maſs, to have loſt its metallic brilliancy and weight, and to have paſſed into the ſtate of a black oxyde. Similar phenomena take place in treating ſteel after the ſame manner. Crude iron is leſs ſuſceptible of changing its metallic fracture by ſuch treatment; though it is rendered very infuſible by the expulſion of its carbon.

Mr. C. ſays that "blaſt furnaces are like two four-ſided pyramids joined baſe to baſe;" Mr. Chaptal ſays the ſame thing, and ſuch I queſtion not may be their ſhape in France. In Britain however they are entirely conical, excepting 6 or 7 feet at the under end, which forms a ſquare funnel: a more juſt idea of the figure would be conveyed by comparing them to two inverted cones, the baſe of the larger reſting upon that of the ſmaller.

In *charging* the furnace Mr. Collier has forgot to mention the principal material, and its proportions, viz. the *ore*, and erroneouſly ſtates, that "equal portions of coal cinder and lime-ſtone" are introduced, without even mentioning whether theſe proportions are by meaſure or weight. The proportions of ore, coal, and lime-ſtone, are very various at different works; the whole depending upon an intimate union and knowledge of the quantity and quality of the various principles and ſubſtances contained in each: the proportion of materials beſides is varied at pleaſure, to produce the quality of crude iron wanted.

At ſome works the quantity of lime uſed is 1 to 3 of cinders by weight, but this is an exceſſive proportion, and is only neceſſary to correct the impurities of ſome iron-

ſtones;

ſtones; in common 1 to 4 or 5 is uſed, and where Cumberland or Lancaſhire ore is ſmelted in contact with wood-charcoal, $\frac{1}{20}$th will abundantly ſuffice.

From Mr. Collier's deſcription, a perſon unverſed in the mode of conducting blaſt furnaces would be apt to conceive that the furnace was firſt charged, and then kindled to effect reduction: this of courſe conveys an idea that frequent filling and kindling are neceſſary. The furnace is firſt ſufficiently heated with coal cinder alone; the charging then commences, and continues hourly for a great length of time—two, three, and four years: inſtances have been of a furnace continuing to ſmelt, without much interruption, for nine years.

The mention of bellows is a proof how deficient Mr. Collier's information has been reſpecting the preſent general ſtate of machinery attached to iron works. Excepting a very few charcoal furnaces, all the blaſt furnaces in Britain are ſupplied with air from caſt-iron cylinders of various dimenſions, conſtructed on various plans. The compreſſion of air from theſe is greater than could poſſibly be obtained by means of bellows, more fit for combuſtion, and capable of ſupporting a column of mercury of ſix inches, which is equal to 6,784 feet of water. By ſuch a heavy compreſſion of air, 1500 to 1800 cubical feet will paſs through the diſcharging pipe into the furnace in one minute, when its diameter does not exceed $2\frac{3}{4}$ or 3 inches at moſt.

Mr. Collier writes with more information on the manufacturing of bar iron. Still however he does not diſtinguiſh, with ſufficient accuracy, between the various proceſſes. The ſecond method he deſcribes is only followed out where bar iron is manufactured with charred wood: the effects produced by the uſe of wood-charcoal are ſo material, that they undoubtedly merited to be mentioned, and particularly diſtinguiſhed from thoſe of charred or raw pit-coal.

One of the principal operations, well known by the name of the Puddling Proceſs, is very ſlightly noticed, and in a manner

manner not eaſily to be underſtood. Dr. Beddoes has lately given a more accurate detail of the various phenomena exhibited during that operation*.

The ſubſtitution of large rollers, in place of the ſtamping-hammer, is well calculated to overcome a large quantity of iron, and for impoſing on the bar a beautiful finiſh, eſpecially if afterwards paſſed under a ſmall tilt-hammer; but the quality of the iron muſt ſtill be inferior to that manufactured with heavy hammers. The compreſſion of the bar, on all ſides, when paſſing through the grooved rollers, muſt tend to enfold, rather than expel, a portion of the ſlug; whereas the heavy blows of a hammer, which with the helve weighs from 3 to 4000lb. cannot fail more entirely to diſſipate all the ſubſtances fuſed in contact with the iron, and to give a degree of fibre and tenacity not to be acquired by a regular and continued preſſure.

Numerous and diverſified are the opinions reſpecting the exiſting cauſes, which conſtitute the very great difference, in quality, between the bar iron manufactured in Great Britain, and that made in Sweden, Siberia, Spain, &c. I have reaſon to imagine that this difference ariſes neither from the ruſt, acquired by the iron being laid in damp places, nor yet from the ſuperior richneſs of the foreign ores. In Siberia and Sweden the iron is puſhed off to market at ſtated periods, and becomes comparatively old kept, merely from the great diſtance it has to travel before it can reach a market. Mr. Collier ſeems inclined to impute this ſuperiority to a greater quantity of iron contained in the ore; and to its being more deſtitute of mixtures: hence he ſtates the average of the Swediſh ores to be 70 pounds pure iron—crude iron he muſt mean—in the hundred, and Engliſh ores 30 to 40. I have ſeen many of the northern ores, ſome of them very rich in iron, equal to 65 and 70 per cent.; but many more of them not yielding beyond 40, and ſome of them as low as 20. Norwegian ore from the mine of Horke-

* Dr. Beddoes's paper ſhall be given in a future Number.

Dhal-

Dhal-Grube is powerfully obedient to the magnet, yet yields only 21 pounds in 100 of the ore.

Admitting, however, that rich ores make a ſuperior quality of iron, why then are not ſuch qualities produced from the ores found in Cumberland or Lancaſhire, ſince the hæmatites and kidney of the former are as rich in iron as the fineſt Swediſh ores? Mr. Collier ſhould have diſtinguiſhed, too, between the argillaceous ores, commonly called iron-ſtones, found in ſtrata more or leſs inclined to the horizon, ſo univerſally diffuſed and ſmelted; and thoſe of Cumberland and Lancaſhire, formed into knobs, or in vertical ſtrata, which are only partially uſed. The former are the only ores uſed in quantity to produce grey crude iron, and contain of iron from 18 to 42 pounds in 100: the latter are uſed in great part for fabricating the inferior qualities of crude iron, ſuch as ſhip ballaſt, forge pigs, &c. &c. The Cumberland ore exhibits a variety of qualities and proportions of iron:

The fine Hæmatites ore yields of iron 68 to 70lbs. per 100.

Kidney - - - 60 to 63.

Common - - 53 to 60.

Lancaſhire ore will be found to yield, upon an average, 57 pounds iron in 100.

I ſhould be apt to aſcribe the general ſuperiority of foreign iron over Britiſh, ſolely to the uſe of wood charcoal in all the ſtages of the operation; and the ſuperiority of the individual fabrics over each other, to the native excellence of the iron contained in the ore. It is well known that the Swediſh marks of bar iron, (L) and O——O, are manufactured from a peculiar ore, found in the mine of Danamora; and that no other Swediſh ores yield ſuch a quality of iron, even when manufactured in the ſame manner and with the ſame fuel. It is the ſame in Siberia, where the old and new ſable fabrics have gained, by the native ſuperiority of the iron contained in the ore, an excellence little inferior to the principal Swediſh marks. From numerous experiments I am convinced, that the iron contained in our iron-ſtones, or

argillaceous ores of iron, is in many of a ſuperior quality, and capable of producing bar iron equal to foreign, if treated in the ſame manner: this, however, is out of the queſtion in Britain, and can never be a ſubject of general contemplation; though by thoſe who have opportunity the experiment might be made.

Moſt of the bog-ores of Sweden and Ruſſia contain leſs iron than ſome of our iron-ſtones, and give out a much leſs portion of volatile matter in torrefying; ſo that ſuch iron-ſtones, by ſevere torrefaction, might be reduced to contain as much iron in 60 or 63 parts as in 100 when in a raw ſtate. Several of the fine iron-ſtones found in Lanarkſhire are compoſed as follows:

Iron - - -	40	
Carbonic acid, ſulphur, &c.	38,	which flies off in torrefying.
Clay, lime and ſilex -	22	
	100 parts:	

ſo that in 100 pounds of iron-ſtone thus roaſted, there would be contained of iron 64½ pounds. Ores in ſuch a ſtate, and containing ſuch a quantity of iron, would moſt likely ſmelt to great advantage in contact with wood-charcoal. In diſtinguiſhing the qualities of crude iron, Mr. Collier has copied the French writers, without adverting to the difference between the fracture of crude iron manufactured with pit-coal, and that produced with the charcoal of wood; this laſt is that deſcribed by the French metallurgiſts. Crude iron manufactured with pit-coal is either white, mottled, or grey: of the latter colour there are many ſhades; the darker the grey, and the larger the grain, the ſofter and more fuſible is the quality of the iron. Theſe properties are expreſsly derived from, and dependent upon, the proportion of fuel uſed in reduction; the greater the quantity of fuel, the larger and more brilliant will be the grains of the fracture of the metal. The carbon contained in crude iron—for *all* crude iron contains carbon—will be in exact proportion to the relative quan-

tity of fuel made uſe of. Mr. Collier aſſerts, that the ſecond ſtate of the metal only contains it; and although he admits of a third ſtate of crude iron, manufactured with a ſtill larger proportion of fuel, he takes no notice of the invariable conſequence, viz. an extra combination of carbon. Query, Can it be ſaid with propriety, that crude iron holds carbon in ſolution, when it can be ſeparated from it by ſimple mechanical diviſion?

It is to be wiſhed that Mr. Collier had furniſhed an analyſis of thoſe ores, found in the neighbourhood of Sheffield, which he ſays contain phoſphat of iron, and from which they derive their cold ſhort quality: if the property of forming cold ſhort irons is a proof of the exiſtence of phoſphat of iron in the ore, then I know of no ores that do not contain it; as I have never ſeen crude iron that could not be converted into cold ſhort iron, at pleaſure, by the management of the proceſs of malleability. I would not wiſh to deny the exiſtence of ſiderite—I cannot—for it is already an eſtabliſhed truth; but I cannot ſubſcribe to its being the exiſting, alterative principle in the ore, which contaminates the iron, and renders it brittle when cold; unleſs I am convinced, by its having been *precipitated from the ore*, before it came in contact with combuſtion. The great Lavoiſier himſelf appears to have been uncertain, whether the ſubſtance called ſiderite was a binary combination or not: hence he claſſes it among the phoſphurets, and expreſſes his doubts of the oxygenation of the phoſphorus. Mr. Chaptal, however, in experiments, which I ſuppoſe have been made poſterior to Mr. Lavoiſier's publication, has proved it to be a real ſolution of iron in the phoſphoric acid. What I contend for is this, that the ſiderite of iron may be an unknown combination, formed during the proceſs of making the iron malleable. At ſome future period I ſhall enter more at large into what *I conceive* to be the exiſting cauſes of cold and ſhort hot irons, and adduce experiments for their ſupport.

The

The account of caſe-hardening is very unſatisfactory; and although the analogy betwixt it and the cementing of ſteel be great, yet it would puzzle any one to make bar ſteel from iron by following the proceſs deſcribed; and ſtill more to produce bar ſteel from caſt-iron, by cementing it in the manner mentioned by Mr. Collier.

I hope Mr. Collier will not feel offended by the freedom with which I have examined his obſervations. Great are the improvements wiſhed for, and expected, in the manufacturing of iron; and it is only by numerous experiments, and liberal diſcuſſions on the ſubject, that theſe great deſiderata are to be obtained.

III. *Obſervations on Platina, and its Utility in the Arts, together with ſome Remarks on the Advantages which reflecting have over achromatic Teleſcopes. By* ALEXIS ROCHON, *Director of the Marine Obſervatory at Breſt. From the* Journal de Phyſique, 1798.

PLATINA is a metal exceedingly refractory, unchangeable, very compact, and capable of receiving a fine poliſh. This ſingular metal has never yet been found but in the gold mines of Choco. The Spaniards gave it the name of *juan blanca*, that is to ſay white gold, and *platino del Pinto*, which ſignifies little ſilver of Pinto. It is brought to us from Choco, under the form of triangular grains the angles of which are rounded. Theſe grains are irregular, ductile, and ſuſceptible of being attracted by the loadſtone. It is never pure, and always contains a black ſhining ſand, over which an artificial magnet has great power. This ſand is interſperſed with gold grains and fragments of ſmall coloured cryſtals. The ſpecific gravity of platina is to that of gold as 22 to $19\frac{1}{2}$*. Like that precious metal, it reſiſts the action of ſimple acids,

* We have ſeen ſome platina where the difference was ſtill greater. EDIT.

and its colour inclines a little more to that of silver than to that of iron.

During the time I resided at Corunna in 1770, the intendant general of Galicia, Pietra Buena, who came originally from Saint-Malo, made me a present of a small ingot of platina, which was combined with a great deal of zinc and red copper. This ingot, which weighed about eight ounces, was in Spain considered as a very rare and curious specimen. Buffon afterwards gave me two dishes of a mixture altogether similar, which had been manufactured at Lima. If platina be thus manufactured in Peru, it needs excite no wonder that travellers have not thought it worth while to bring to Europe works made by a mixture which deprives this metal of the advantages it has in its natural state over all other metallic substances.

It is believed that platina was not known in Europe before the appearance of Don Antonio de Ulloa's Voyage, printed in 1748: but it appears that it was known to Julius Scaliger; for he mentions a similar metal in one of his works printed at Franckfort in 1601. "Know also," says he, "that in Honduras, a district between Mexico and Darien, there are mines which contain a metal never yet brought to a state of fusion by fire, or by any of the arts employed by the Spaniards*." Scaliger then knew that there had been discovered, between Mexico and the isthmus of Darien, a metal so refractory that the Spaniards had never been able to melt it. He made use of this as an argument against Jerome Cardan, whom he attacked on account of the definition he had given of metals. This passage, which is curious and decisive, was pointed out to me by Izquierdo, director of his Spanish majesty's cabinet, who is not only an able naturalist, but well acquainted with the arts, to which he is a friend and a patron.

* Præterea scito, in Funduribus, qui tractus est inter Mexicam et Darien, fodinas esse aurichalci, quod nullo igne, nullis Hispanicis artibus, hactenus liquescere potuit.

In

In the Memoirs of the Swediſh Academy for 1751, there is an account of ſeveral experiments on platina, by Theodore Scheffer; but the moſt intereſting of theſe is one which proves that this metal may be reduced to complete fuſion when united with a very ſmall quantity of arſenic. William Lewis, who made a valuable ſeries of experiments on the ſame ſubject, would not repeat the experiment of Scheffer, becauſe it appeared to him abſurd to reduce a refractory metal to a ſtate of fuſion, by means of a ſubſtance which a moderate heat ſublimes and volatiliſes. The illuſtrious Turgot, whoſe talents and virtues, ſuperior to the age in which he lived, will be handed down to the remoteſt poſterity, ſent to Peru during his adminiſtration the celebrated botaniſt Dombei. This voyage, the object of which comprehended great and enlarged views, was of equal importance to the arts and the ſciences; and Dombei, according to the inſtructions given him, was to ſpare no pains to procure for men of ſcience ſuch a quantity of platina as might be uſeful to them in their reſearches. Turgot, whoſe knowledge in natural philoſophy was profound and extenſive, had foreſeen that a metal indeſtructible by its nature would be of ineſtimable value in the conſtruction of teleſcopes, and of thoſe inſtruments which navigators employ at ſea to determine the longitude by the diſtance of the ſun and ſtars from the moon. All opaque bodies are not equally proper for reflecting the light in a regular manner, and indeed thoſe which poſſeſs this uſeful property are very few in number. Tempered ſteel, pyrites known under the name of the Inca's ſtone, and glaſs covered at the back with tin, cannot be employed in forming ſpecula for teleſcopes, though they reflect the light regularly. The violent friction neceſſary to poliſh tempered ſteel; the nature and imperfections of pyrites; the difficulty of avoiding double refraction in quickſilvered glaſs, are the cauſes which prevent theſe ſubſtances from being uſed. Opticians, therefore, have hitherto been obliged to employ for this purpoſe a metal com-

poſed of arſenic, red copper, and tin, the proportions of which they vary to a certain degree. This metal, which is attacked by all acids, ſoon tarniſhes when expoſed to the air. To preſerve the poliſh neceſſary to theſe ſpecula, requires a care and attention which few people can beſtow; and the great ſuperiority of reflecting over refracting teleſcopes has not induced aſtronomers to give them the preference, as it is exceedingly difficult to guard them from ruſt, eſpecially when they are frequently uſed in the night-time.

Herſchel, however, did not entertain the ſame apprehenſions; and by means of a large reflecting teleſcope, that celebrated aſtronomer was enabled to diſcover the planet which bears his name. With this inſtrument alſo he made other intereſting diſcoveries. Herſchel, we are aſſured, employs magnifying powers which aſtoniſh all aſtronomers; but when they ſhall employ the like means to examine the ſtars, their aſtoniſhment may perhaps be leſſened. We are indeed taught by experience, that the image of an object is never very diſtinct, unleſs it occupy on the retina the ſpace of half a minute; and we may aſſure ourſelves of this, by looking at Jupiter through a good reflecting or refracting teleſcope, which magnifies the diameters of objects more than a hundred times. The diſk of Jupiter, which will be well defined on being magnified to this degree, ceaſes to be ſo when the magnifying power of the teleſcope is reduced to four or five times. This fact is confirmed by a ſeries of experiments made at Calais by my colleague Fourcroy. It is then evident, that extraordinary magnifying powers are more favourable than hurtful in obſervations made on objects the diameters of which are infinitely ſmall.

If the diſcoveries of Herſchel were not ſufficient to ſhew the great ſuperiority of reflecting over refracting teleſcopes, it would not be difficult to call in theory to the aid of experience; and it might be proved, that the aberration of refrangibility, which cannot take place in reflectors, is never entirely removed in the beſt achromatic teleſcopes. The

laws

laws of reflection are invariable, but those of refraction and diſperſion are ſuſceptible of variations. The ſubſtance of glaſs is never perfectly homogeneous. Reflection, optical illuſions, and the loſs of light, are unavoidable in achromatic teleſcopes, on account of the multiplicity of glaſſes employed in their conſtruction; but the ſpecula of teleſcopes, whatever be the nature of the ſubſtance of which they are compoſed, are not liable to the ſame faults.

In ſpeaking of achromatic teleſcopes, it is impoſſible not to recollect, that the immortal Euler was led to the diſcovery of them by a reaſoning purely metaphyſical. By reflecting on the ſtructure of the eye, this great geometrician concluded, that the Supreme Being had compoſed that organ of different humours, only for the purpoſe of deſtroying the aberration of refrangibility. If the metaphyſical ideas of Euler on the ſtructure of the eye do not accord with the numerous experiments which my colleague Tenon enabled me to make on the humours which compoſe that organ; the inference he drew from them was not leſs happy, ſince it ſuggeſted to him the plan of making achromatic teleſcopes by employing two different refracting ſubſtances. The Engliſh exclaimed at firſt againſt this idea of Euler; but John Dollond, who at firſt oppoſed that eminent mathematician's theory, found in the great diſperſion of flint glaſs the means of realiſing it. The nature of the reſearches of that ingenious optician, and the advantages he derived from them, have well entitled him to ſhare with Euler in the glory of this noble and important diſcovery.

The glaſs of lead is not the only kind that cauſes a ſtrong diſperſion; for ſeveral other kinds of metallic glaſs poſſeſs the ſame uſeful property. Some philoſophers have imagined that there is a conſtant relation between the ſpecific gravity of glaſs and its diſperſion; but it is to be remarked, that the diſperſion which takes place in ether and ſpirit of wine is ſtronger than that produced in water, which is a much heavier liquor. Though opticians hitherto have employed

only flint and common glaſs in the conſtruction of achromatic teleſcopes, it is certain that the ſame object might be attained with other tranſparent ſubſtances, both ſolid and fluid*; but, as the queſtion propoſed by the Academy has in view only the improvement of flint glaſs, I ſhall here offer ſome obſervations, which may be of uſe to thoſe who wiſh to become candidates for the prize†.

Flint glaſs is brought to us from England in very thin plates. It is blown into globes, which are cut up and ſtretched out when they have attained to the proper ſize and thickneſs. Blown glaſs will conſiſt of parallel layers, if the workman is not able to take up at once the neceſſary quantity of matter; and theſe layers, the junction of which is

* This idea has been realiſed by Dr. Blair of Edinburgh. Flint glaſs reflects the green light conſiderably leſs than crown glaſs, in proportion to the whole refraction of red and violet light; ſo that when the divergency of the red and violet light cauſed by the refraction of the two mediums is equal, the divergency of the red and green light is always greater in the crown than in the flint glaſs, and the divergency of the violet and green light is always leſs in the crown than in the flint glaſs. After a variety of experiments, Dr. Blair diſcovered that the muriatic and nitric acids, which are diſperſive fluids of conſiderable ſtrength, inſtead of refracting the green light leſs than crown glaſs, in proportion to the whole refraction of the red and violet light, refracted the green light more than crown glaſs in proportion to the whole refraction of red and violet light; he therefore mixed theſe two kinds of diſperſive mediums, and thus obtained a medium which diſperſes the rays much more than crown glaſs, and yet cauſes them all to diverge accurately in the ſame proportion in which they are made to diverge by the refraction of crown glaſs; which entirely removes the aberration from the unequal refrangibility of light. For this diſcovery Dr. Blair has obtained a patent. EDIT.

† This memoir, the author tells us in a note, was read on his public admiſſion into the Academy of Sciences at Paris, in the year 1786. It was printed in the ſecond part of his *Voyage to Madagaſcar*; but that part, owing to an accident which could neither be foreſeen nor prevented, was never given to the public. It contained nautical details, obſervations on the charts of India, and a diſſertation on the inſtruments which may be uſeful or neceſſary for the navigator.

rarely

rarely perfect, will form laminæ, that may be eaſily obſerved on looking at the edge of the glaſs. Undulating threads are almoſt always found alſo at the joinings of the layers. Opticians diſtinguiſh two kinds of theſe threads, the firſt of which they call ropes, becauſe they are full and of different denſities: they are always prejudicial to the goodneſs of optical inſtruments. The ſecond are leſs troubleſome, but more common. They are capillary tubes, which produce two pencils of light in a direction perpendicular to their axes.

I have already given, in a work entitled *Memoires ſur la Mecanique et la Phyſique*, a method of obtaining large object glaſſes from thin plates of flint glaſs, and of freeing them from threads when their diſpoſition in the ſubſtance of the glaſs does not render it impoſſible. This proceſs, which I have ſtill brought to greater perfection, conſiſts in expoſing the glaſs to a fire ſufficiently ſtrong to melt it. When the glaſs is ſoft, it muſt be rolled up with a pair of iron pincers, and formed into a cylinder three inches in thickneſs. Thoſe folds which would infallibly reſult from too rapid bending ought to be carefully avoided. The edges of the glaſs muſt be kept in by a ring of clay. The cylinder muſt then be left in the furnace a ſufficient length of time to be annealed; and when this operation is terminated, it muſt be poliſhed, in order to diſcover its faults. It is then to be ſawn through at the places moſt filled with threads, and to be put into the furnace a ſecond time to give it the proper form.

The detail into which I have entered on the nature of glaſs proper for optical purpoſes, ought not to make us loſe ſight of the great advantages which reflectors have over achromatic teleſcopes. Since the diſcoveries of Herſchel, the utility of the former cannot be doubted; but it is of great importance to employ, in the conſtruction of theſe inſtruments, a metal the poliſh of which may be bright and unchangeable. Twenty pounds of this precious metal, which

were

were preſented to me by the miniſter of the marine, on hearing an account given of the utility of my reſearches, have greatly contributed to their ſucceſs. Opticians who ſtudy the nature of thoſe ſubſtances uſed in the compoſition of ſpecula, know that no metal or ſemi-metal can ſupply the place of tin. It is tin which renders red copper brittle, *elaſtic*, and ſuſceptible of a fine poliſh. This metal produces alſo the ſame effects on all other metallic ſubſtances. Platina is ſubjected to the ſame law. But I ſhall not here relate the numerous trials I was obliged to make before I could obtain a ſatisfactory reſult. I ſhall confine myſelf to an account of that proceſs, which was attended with the greateſt ſucceſs.

The platina in grains muſt be purified in a ſtrong fire by means of nitre and the ſalt of glaſs*. To the platina, when purified, add the eighth part of that metal employed in the compoſition of common ſpecula; for tin without red copper would not produce a good effect. This mixture is then to be expoſed to the moſt violent heat, which muſt be ſtill excited by the oxygen gas that diſengages itſelf from nitre when thrown into the fire. One melting would be inſufficient: five or ſix are requiſite to bring the mixture to perfection. It is neceſſary that the metal ſhould be in a ſtate of complete fuſion at the moment when it is poured into the mould. By this proceſs I have been enabled to conſtruct a teleſcope with platina, which magnifies the diameters of objects five hundred times, with a degree of clearneſs and diſtinctneſs requiſite for the niceſt obſervations. The large ſpeculum of platina weighs fourteen pounds: it is eight inches in diameter, and its focus is ſix feet. This ſpeculum, which is unchangeable, was caſt and poliſhed by Carrochez.

The utility of platina in the arts is not confined to the conſtruction of teleſcopes and nautical inſtruments. Robin, a celebrated watch-maker, wiſhed to employ it in making

* The flux uſed in the Engliſh glaſs-houſes, and called by the workmen *ſandiſer*. EDIT.

for

for me eſcapements and compenſation balances. He is of opinion, that this metal is extremely proper for the holes in which the pivots move, becauſe it preſerves oil without the leaſt alteration. Platina has no occaſion, like gold, to be combined with any other metal, to render it fit for being uſed in the arts. This new metal afforded me excentric watch dial-plates ſuperior to thoſe brought from England. Cotteau, an ingenious enameller, who himſelf made various experiments with platina, told me, that for his art he preferred it to all other metals. Platina is alſo a metal of ineſtimable value for conſtructing different inſtruments of chemiſtry. I have formed it into crucibles*.

The experiments which I lately made at Saint Gobin with Deſlandes, director of the plate-glaſs manufactory, do not permit me to doubt of the utility of crucibles made of platina in bringing flint-glaſs to perfection; and Deſlandes, who has been able to give to the glaſs of Saint Gobin a ſuperiority which foreigners cannot diſpute, is of my opinion in regard to crucibles of this metal. Crucibles indeed of earth, whatever be its nature, become in part vitrified. The glaſs which thence reſults is imperfect; and it may be ſeen by the naked eye, that the vitreous ſubſtance contained in the crucible does not form one homogeneous maſs. It may be readily perceived, that crucibles of platina cannot be attended with the ſame inconvenience.

[To be concluded in the next Number.]

* Since this memoir was read, I cauſed to be caſt a crucible of platina capable of containing thirty pounds of flint-glaſs; and to give it more ſtrength, it is covered on the outſide with a caſe of caſt iron an inch in thickneſs. In the latter operation I was aſſiſted by my colleague Perrier.

IV. *Cit. CHAPTAL's Proceſs for whitening Prints, printed Books, and other Articles of Paper.*

SIMPLE immerſion in oxygenated muriatic acid, letting the article remain in it a longer or ſhorter ſpace of time, according to the ſtrength of the liquid, will be ſufficient to whiten an engraving. If it be required to whiten the paper of a bound book, as it is neceſſary that all the leaves ſhould be moiſtened by the acid, care muſt be taken to open the book well, and to make the boards reſt on the edge of the veſſel, in ſuch a manner that the paper alone be dipped in the liquid: the leaves muſt be ſeparated from each other, in order that they be equally moiſtened on both ſides.

The liquor aſſumes a yellow tint, and the paper becomes white in the ſame proportion. At the end of two or three hours the book may be taken from the acid liquor and plunged into pure water, with the ſame care and precaution as recommended in regard to the acid liquor, that the water may exactly touch the two ſurfaces of each leaf. The water muſt be renewed every hour to extract the acid remaining in the paper, and to diſſipate the diſagreeable ſmell.

By following this proceſs, there is ſome danger that the pages will not be all equally whitened, either becauſe the leaves have not been ſufficiently ſeparated, or becauſe the liquid has had more action on the front margins than on thoſe near the binding. On this account the practice followed by book-binders, when they wiſh to whiten printed paper, is to be preferred. They deſtroy the binding entirely, that they may give to each leaf an equal and perfect immerſion; and this is the ſecond proceſs recommended by M. Chaptal.

"They begin," ſays he, "by unſewing the book and ſeparating it into leaves, which they place in caſes formed in a leaden tub, with very thin ſlips of wood or glaſs, ſo that the leaves when laid flat are ſeparated from each other by intervals

tervals ſcarcely ſenſible. The acid is then poured in, making it fall on the ſides of the tub, in order that the leaves may not be deranged by its motion. When the workman judges, by the whiteneſs of the paper, that it has been ſufficiently acted upon by the acid, it is drawn off by a cock at the bottom of the tub, and its place is ſupplied by clear, freſh water, which weakens and carries off the remains of the acid, as well as its ſtrong ſmell. The leaves are then to be dried, and, after being preſſed, may be again bound up.

"The leaves may be placed alſo vertically in the tub; and this poſition ſeems to poſſeſs ſome advantage, as they will then be leſs liable to be torn. With this view I conſtructed a wooden frame, which I adjuſted to the proper height, according to the ſize of the leaves which I wiſhed to whiten. This frame ſupported very thin ſlips of wood, leaving only the ſpace of half a line between them. I placed two leaves in each of theſe intervals, and kept them fixed in their place by two ſmall wooden wedges, which I puſhed in between the ſlips. When the paper was whitened I lifted up the frame with the leaves, and plunged them into cold water to remove the remains of the acid, as well as the ſmell. This proceſs I prefer to the other.

"By this operation books are not only cleaned, but the paper acquires a degree of whiteneſs ſuperior to what it poſſeſſed when firſt made. The uſe of this acid is attended alſo with the valuable advantage of deſtroying ink ſpots. This liquor has no action upon ſpots of oil, or animal greaſe; but it has been long known that a weak ſolution of potaſh will effectually remove ſtains of that kind.

"When I had to repair prints ſo torn that they exhibited only ſcraps paſted upon other paper, I was afraid of loſing theſe fragments in the liquid, becauſe the paſte became diſſolved. In ſuch caſes I encloſed the prints in a cylindric glaſs veſſel, which I inverted on the water in which I had put the mixture proper for extricating the oxygenated muri-

atic

atic acid gas. This vapour, by filling the whole inſide of the jar, acted upon the print; extracted the greaſe as well as ink ſpots; and the fragments remained paſted to the paper."

Method of preparing the Oxygenated Muriatic Acid.

To oxygenate the muriatic acid, nothing is neceſſary but to dilute it, and mix it in a very ſtrong glaſs veſſel with manganeſe, in ſuch a manner that the mixture may not occupy the whole content of the glaſs. Air bubbles are formed on the ſurface of the liquor; the empty ſpace becomes filled with a greeniſh vapour; and, at the end of ſome hours, the acid may be farther diluted with water and then uſed. It has an acid taſte, becauſe the whole is not ſaturated with oxygen; but it poſſeſſes all the virtues of the oxygenated muriatic acid. This proceſs may be followed when there is not time to ſet up an apparatus for diſtilling, in order to procure the oxygenated acid.

V. *Proceſs for removing Spots of Greaſe from Books and Prints. By M.* DESCHAMPS, *Jun. Member of the Philoſophical Society at Lyons. From* Bibliotheque Economique, *Vol. I.*

A TASTE for elegant editions, books in good preſervation, and proof impreſſions of prints, can be conſidered as a mania only by thoſe who are unacquainted with literature. In a well printed, carefully preſerved, and neat book, the ſenſe ſeems to paſs through the organs of ſight, in order to meet the underſtanding; while, in a bad, confuſed edition, or a dirty, ſtained and diſguſting copy, the confuſion of the characters deranges, as it were, the connection of the author's thoughts; their obſcurity diveſts ideas of their brilliancy;

brilliancy; and the dirtineſs of the paper, which offends the eye, makes the ſubject loſe much of its charms and attraction. It is a great misfortune, therefore, to thoſe who purchaſe books for the ſake of reading them, when the objects of their enjoyment are injured by the too cloſe application of a candle or lamp. For this reaſon M. Deſchamps, at the requeſt of a man of letters, to whom an accident of this kind had happened, deviſed the following proceſs, which was attended with complete ſucceſs:

After having gently warmed the paper ſtained with greaſe, wax, oil, or any fat body whatever, take out as much as poſſible of it, by means of blotting-paper. Then dip a ſmall bruſh in the eſſential oil of well rectified ſpirit of turpentine, heated almoſt to ebullition (for when cold it acts only very weakly), and draw it gently over both ſides of the paper, which muſt be carefully kept warm. This operation muſt be repeated as many times as the quantity of the fat body imbibed by the paper, or the thickneſs of the paper, may render neceſſary. When the greaſy ſubſtance is entirely removed, recourſe may be had to the following method to reſtore the paper to its former whiteneſs, which is not completely reſtored by the firſt proceſs. Dip another bruſh in highly rectified ſpirit of wine, and draw it, in like manner, over the place which was ſtained, and particularly round the edges, to remove the border, that would ſtill preſent a ſtain. By employing theſe means, with proper caution, the ſpot will totally diſappear; the paper will reſume its original whiteneſs; and if the proceſs has been employed on a part written on with common ink, or printed with printers ink, it will experience no alteration.

VI. *Obſer-*

VI. *Obſervations on the Mole, and the Means of extirpating that deſtructive Animal. From* Economiſche Hefte, *Vol. VII. Part* 5. *and Vol. IX. Part* 4.

THE great damage which moles occaſion in cultivated land, and particularly in gardens, is well known; and the beſt means of remedying this evil is by deſtroying all thoſe that make their appearance, as far as poſſible. The ſecrets which quacks ſell for extirpating theſe deſtructive animals are of very little avail; and even poiſon produces no effect, as the mole does not drink, and lives only on roots and worms. In regard to gins and traps, the moles muſt be enticed to them by ſome kind of bait, which does not always produce the intended effect. Buffon adviſes a trench to be dug around the hills under which they conceal themſelves, and thus to cut them off from all communication with the neighbouring ground. This method requires three or four people to dig trenches; and though it may prove effectual, it is attended with too much trouble. The other methods propoſed by different naturaliſts are neither eaſier nor more certain *.

It is well known that this animal lives under the earth; and if at any time it comes forth from its holes, it is only when compelled to do ſo, in conſequence of large quantities of water accumulated after the heavy rains which fall in ſummer, or when the earth is ſo much parched and dried by the continued drought, that it can no longer continue its labour; but it again creeps back into the earth when it finds a ſpot convenient for its purpoſe.

This animal, as already remarked, feeds upon roots and worms, and for this reaſon is generally found in rich fertile ſoil; but never in that which is marſhy or ſtony. In the

* Valmont de Bomare recommends ſuffocating them in their holes by means of ſulphur. EDIT.

winter

winter time it retires to elevated places, becauſe it is there beſt ſecured from inundations. In ſummer, however, it deſcends to the low hillocks and flat land, and above all makes choice of meadows for the place of its reſidence; becauſe it finds the earth there freſher and ſofter for it to dig through. If the weather continues long dry, it repairs to the borders of ditches, the banks of rivers and ſtreams, and to places contiguous to hedges.

The mole breeds generally at the beginning of winter, and the months when they are found big with young are January and February. In the month of April a great many of their young may be ſeen. Among 122 caught in the month of May by my method, there were only four big with young. This animal cannot live without digging; it is obliged to find its nouriſhment in the bowels of the earth; and on that account is under the neceſſity of making thoſe long ſubterranean paſſages which are found between one mole-hill and another. In general it begins to dig five or ſix inches under the ſurface; it ſcrapes the earth before it on one ſide till the quantity becomes too great for it to labour with eaſe; it then works towards the ſurface; and by puſhing with its head, and the aſſiſtance of its nervous paws, gradually raiſes up the earth which incommodes it, and which produces thoſe ſmall hills ſo common in fields. After getting rid of the earth in this manner, it proceeds forwards, and continues its labour as before. The farther it goes the more hills are produced. At each period of its labour it throws up four or five.

In places overgrown with graſs and ſhrubs, the mole is often contented with only forcing a paſſage through between the roots; and when the earth in gardens has been newly watered, it keeps itſelf at the depth of ſcarcely half an inch under the ſurface. This animal ſhews an equal averſion to great cold and violent heat; and in order to avoid both, it forces its way, when either prevail, to the greateſt depth in the earth.

It continues its labour at all times, becauſe it is neceſſary for it to procure nouriſhment. It is abſolutely falſe that it ſleeps throughout the winter, as ſome naturaliſts have aſſerted; for it throws up the earth in the coldeſt ſeaſon, as well as during the ſummer. It is moſt buſily employed towards the end of winter, and at that period forms the greateſt number of hills. To this it is impelled by more than one reaſon. In the firſt place, it muſt provide nouriſhment for its young; ſecondly, it finds it eaſieſt at that time to dig its way through the earth; and laſtly, as the air begins to be milder, the animal then recovers that ſtrength which it had loſt during the intenſe cold. At this ſeaſon, therefore, it is moſt proper to purſue means for extirpating this animal, as it can be deſtroyed with greater eaſe while employed at its labour.

The male is much ſtronger than the female, and the hills thrown up by the former are much larger as well as more numerous. The periods when the mole is moſt buſily employed in digging are in the morning, at ſun-riſe, at noon, and at ſun-ſet. In dry weather moles are obſerved to throw up the earth for the moſt part only at ſun-riſe, and in winter when the earth has been ſomewhat heated by the ſun's rays.

A perſon may eaſily diſcover how many moles are contained in a certain ſpace of ground, by counting the freſh raiſed mole-hills which have no communication with each other. I muſt remark alſo, that this animal has very bad ſight, being almoſt totally blind; but its hearing, on the other hand, is ſo much the more acute.

I ſhall now proceed to the method of deſtroying them. Immediately at day-break it will be neceſſary to make a tour round the garden or meadow, from which it is wiſhed to extirpate the moles; for at that time they will be all found at work, as may be ſeen by the hills newly thrown up. If the perſon is then cloſe to the hill, he muſt proceed as the gardeners do, and turn up with a ſtroke of the ſpade the hill together with the digger. The paſſage is then cut through

before

before the animal is aware of the attack, and therefore it has not power to eſcape. If the mole-hill be freſh, even though the animal may not be throwing up earth, the perſon ought not to loſe his time in waiting, but ſhould immediately proceed to the operation above mentioned.

If you find a freſh hill ſtanding by itſelf, which ſeems to ſhew by its ſituation that it has no communication with any other, which is always the caſe when the mole has worked from the ſurface downwards in endeavouring to procure a more convenient habitation, after the hill has been turned up with the ſpade, a bucket of water ſhould be poured over the mouth of the paſſage. By theſe means the animal, which is at no great diſtance, will be obliged to come forth, and may be eaſily caught with the hand.

You may diſcover alſo whether a hill has any communication with another, if you apply your ear to it, and then cough or make a loud noiſe. If it has no communication with the neighbouring hills, you will hear the terrified animal make a noiſe by its motion. It will then be impoſſible for it to eſcape; and you may either pour water into the hole, or turn up the hill with a ſpade until the mole is found; for in general it never goes deeper into the earth than from fifteen to eighteen inches.

When any of the beds in a garden have been newly watered, the mole, attracted by the coolneſs and moiſture, readily repairs thither, and takes up its reſidence in them, making a paſſage at the depth of ſcarcely an inch below the ſurface. In that caſe it may eaſily be caught. When you ſee it at work, you need only tread behind the animal with your feet on the paſſage to prevent its retreat, and then turn up the hill with a ſpade; by which means you will be ſure to catch it.

When you dig after it with a ſpade, the animal forces its way downwards into the earth in a perpendicular direction, in order that it may better eſcape the threatened danger. In that caſe it will not be neceſſary to dig long, but to pour

water over the place, which will ſoon make the animal return upwards.

People in general are not aware of the great miſchief occaſioned in fields and gardens by theſe animals. We are however informed by Buffon, that in the year 1740 he planted fifteen or ſixteen acres of land with acorns, and that the greater part of them were in a little time carried away by the moles to their ſubterranean retreats. In many of theſe there were found half a buſhel, and in others a buſhel. Buffon, after this circumſtance, cauſed a great number of iron traps to be conſtructed, by which in leſs than three weeks he caught 1300. To this inſtance of the devaſtation occaſioned by theſe animals, we may add the following: In the year 1742 they were ſo numerous in ſome parts of Holland, that one farmer alone caught between five and ſix thouſand of them. The deſtruction occaſioned by theſe animals is however no new phenomenon. We are informed by hiſtory, that the inhabitants of the iſland of Tenedos, the Trojans, and the Æolians, were infeſted by them in the earlieſt ages. For this reaſon a temple was erected to Apollo Smynthius, the deſtroyer of moles.

VII. *A curious Fact in the Natural Hiſtory of the common Mole,* Talpa Europæa *Linn. By* ARTHUR BRUCE, *Eſq. Secretary to the Natural Hiſtory Society of Edinburgh. From* Tranſactions of the Linnean Society, *Vol. III.* 1797.

THAT the mole does, in common with other quadrupeds and man, poſſeſs that ſpirit of curioſity which prompts to emigration and even to tranſmarine expeditions, I found out laſt ſummer from the beſt authenticated facts.

In viſiting the loch of Clunie, which I often did, I obſerved in it a ſmall iſland at the diſtance of 180 yards from the neareſt land, meaſured to be ſo upon the ice. Upon the iſland,

iſland, lord Airly, the proprietor, has a caſtle and ſmall ſhrubbery. I obſerved frequently the appearance of freſh mole-caſts, or hills. I for ſome time took it to be the water mouſe, and one day aſked the gardener if it was ſo? No, he ſaid, it was the mole; and that he had caught one or two lately. But that five or ſix years ago he had caught two in traps; and for two years after this he had obſerved none. But about four years ago, coming aſhore in a ſummer's evening in the duſk, the 4th or 5th of June, 10 o'clock P. M. he and another reſpectable perſon, lord Airly's butler, ſaw at a ſmall diſtance upon the ſmooth water ſome animal paddling to, and not far diſtant from, the iſland. They ſoon, too ſoon! cloſed with this feeble paſſenger, and found it to be our common mole, led by a moſt aſtoniſhing inſtinct from the neareſt point of land (the caſtle-hill) to take poſſeſſion of this deſert iſland. It was at this time, for the ſpace of about two years, quite free from any ſubterraneous inhabitant; but the mole has for more than a year paſt made its appearance again, and its operations I was witneſs to.

In the hiſtory of this animal I do not, at preſent, recollect any fact ſo ſtriking; eſpecially when we conſider the great depth of the water, both in ſummer and winter—from ſix to ten, fifteen, and ſome places as deep as thirty or forty feet, all round the iſland.

Edinburgh,
April 26, 1793.

VIII. *Experiments and Obſervations tending to ſhow the Compoſition and Properties of Urinary Concretions. By* GEORGE PEARSON, *M. D. F. R. S. Read before the Royal Society, December* 14, 1797. *From the* Philoſophical Tranſactions.

I. *Hiſtorical Obſervations.*

THE notion generally entertained, of the nature of urinary concretions, conſiſted with the names given them *, till the laſt twenty years; although the experiments of Slare, Frederic Hoffman, and Hales, long before ſhowed that theſe ſubſtances commonly conſiſt of animal matter. Galen indeed imagined that φλεγμα, or viſcid animal matter, is the baſis of animal concretions; but, in his days, earth was believed to be the baſis of animal matter. Alkaline medicines were, however, employed by the Greek phyſicians in diſeaſes from calculi.

The experiments of the alchemiſts alſo made it appear, that earth was only a part of the matter of concretions. It was probably the obſervation of the depoſition and cryſtallization of ſaline bodies, which ſuggeſted the notion of urinary calculi being of the nature of tartar. Such was the opinion of Baſil Valentine, and after him of Hochemer, better known by the name of Paracelſus; but, whether the latter adopted the denomination *Duelech* from its import, or from caprice, has not been explained. Van Helmont, a century after his prototype Paracelſus, being ſtruck with the experiment in which he diſcovered the concretion of ſalts in

* Urinary concretions have obtained their denominations, like moſt other things, from their obvious properties. Accordingly, in our language, they are popularly known by the names ſtone and gravel, or ſand, from their reſemblance to the ſtates of earth ſo named: and we find names of the ſame import in other languages, ſuch as λιθος, (ARETÆUS;) λιθιασις, (CÆLIUS AURELIANUS;) ψαμμος, (ARETÆUS;) λιθιδια, (various authors); *calculus*, (CELSUS and PLINY); *ſabulum*, (various authors). In other languages, and eſpecially in thoſe now ſpoken, it is unneceſſary to notice names which have the ſame meaning.

diſtilled

diſtilled urine by alcohol, was led to depart from his adored maſter's opinion, with reſpect to the nature of calculi; although he acknowledges the merit of Paracelſus, in having diſcovered the ſolvent *Ludus*, (a calcareous ſtone alſo called *Septarium*,) which Van Helmont ſays is preferable to alkaline lixivium. He alſo ſays, that when the archeus ſpirit of urine meets with a volatile earthy ſpirit, and does not act in a due manner, a concretion will be formed; but, in a healthy ſtate, although all urine contains the matter of urinary calculi, no concretion can take place, becauſe the archeus, or vital power of the bladder, counteracts its formation.

As to the kind of earth compoſing calculi, the only diſtinction of earths, till about the laſt half century, was into abſorbent and non-abſorbent; but, ſince the abſorbent earths were diſtinguiſhed into calcareous, magneſia, and alumine or clay, the calcareous was conſidered to be the earth of urinary concretions; apparently however for no other reaſon but its obvious properties, and its extenſive diffuſion through the whole animal kingdom.

At length, *viz.* in 1776, the experiments of the wonderful Scheele were publiſhed in Sweden, but were ſcarcely known in this country till 1785. Theſe experiments exploded the opinion of the earthy nature of calculi, and ſubſtituted that of their conſiſting of a peculiar acid, reſembling the ſuccinic, and of a gelatinous matter, without any earth. Afterwards about $\frac{1}{200}$ of their weight of lime was found by Bergman; which, for a cauſe now well known, had eluded the acuteneſs of Scheele. Although the experiments of Scheele were confeſſedly unqueſtionable, and were ably ſupported by the learned Bergman, ſome very eminent chemiſts, having obtained different reſults by their own experiments, adopted a different opinion of the compoſition of theſe concretions. The immortal, and ever to be deplored, Lavoiſier ſuppoſed theſe ſubſtances to conſiſt of acidulous phoſphate of lime and animal matter, many of them being partially

fuſible; but ſtill it was the unrivalled Scheele who diſcovered that the urine of healthy perſons contains ſuperphoſphate, or acidulous phoſphate, of lime; and who alſo indicated the experiment which verified his opinion, that phoſphate of lime is the baſis of bone.

Experiments have been likewiſe made, for the moſt part in a rather deſultory way, and moſt of them by perſons but little practiſed in chemical inquiries, which at leaſt afford evidence, that urinary concretions are very different, with reſpect to the proportion of the ingredients in their compoſition, and perhaps alſo in kind. M. Fourcroy, who however muſt not be claſſed with inexperienced chemiſts, I believe firſt obtained pruſſic acid by fire, and by nitric acid, from theſe concretions; and ſhowed that they ſometimes contain phoſphate of ammoniac and of ſoda; which may be diſſolved out of them by water. M. Fourcroy alſo ſays, he found magneſia in the inteſtinal calculus of a horſe; which calculus was a triple combination, of one part of phoſphate of ammoniac, two parts of magneſia, and one of water, beſides traces of animal and vegetable matter.

Dr. Link, in a very elaborate diſſertation, publiſhed at Gottingen, in 1788, on urine and calculi, concludes that urinary concretions conſiſt of phoſphoric acid, lime, ammoniac, oil, the baſes of different kinds of gazes, together with the acid ſublimate of Scheele, although he did not ſucceed in obtaining it.

It is a proof of Dr. Black's ſagacity, that he ſhould have been able to perceive, from a few experiments, that urinary concretions conſiſted of animal matter and the earth of bone, before the compoſition of this earth was demonſtrated by Gahn.

In this hiſtorical ſketch it ſhould be noticed, that alkaline ſubſtances, though uſed by the Greek phyſicians, and afterwards by the alchemical phyſicians, appear to have been laid aſide by the regular practitioners, for a century or two preceding their revival, by the famous Mrs. Stephens, in 1720.

Her

Her prescription brought into vogue the theory of these medicines operating by their causticity. The successful use, by Mr. Colborne, of potash saturated with carbonic acid, according to the discovery of Bewley and Bergman, and the still further improvement in practice, from the use of soda, as well as potash, super-saturated with carbonic acid, by the discovery of a peculiar method by Mr. Schweppe, have completely refuted the theory of the agency of alkalies on the principle of causticity.

It appears, from the preceding brief history, as well as from the confession of the latest and best writers, that the experiments hitherto made, rather "afford indications of what remains to be done, than furnish demonstrations of the nature of animal concretions." It is also too obvious to need explanation, that more efficacious and innocent practice, in diseases from these concretions, can only be discovered by a further investigation of their properties. It is with this view, as well as for the sake of chemical philosophy, that I think it my duty to submit to the Society some of the observations I have made, in the course of inquiry on this subject.

The observations which I shall now offer, are principally on a substance, which my experiments inform me is very generally a constituent of both urinary and arthritic concretions. It is a substance obtained by dissolving it out of these concretions, by lye of caustic fixed alkali, and precipitating it from the solution by acids. In this way, Scheele separated this matter; but he did not consider its importance, nor of course at all investigate its properties. He does not even seem to have been aware that it was a distinct constituent part of the urinary concretion; for, when he relates the experiment of precipitating matter from the nitric solution of calculus by metallic salts, no distinction is made between the precipitations in this experiment, and that in the former: yet we can now show, that in the one case the precipitate is a peculiar animal oxide, and in the other they are metallic phosphates.

phoſphates. As Scheele obtained an acid ſublimate, it has been imagined by ſome writers, that the precipitate by any acid (even by the carbonic) from the alkaline menſtruum, was an acid; the ſame as that obtained by ſublimation, and which, in the new ſyſtem of chemiſtry, has been denominated *lithic acid*. The following experiments ſhow that theſe ſubſtances are different ſpecies of matter.

II. *Experiments.*

250 grains of a white, ſmooth, laminated, urinary calculus, and the ſame quantity of a nut-brown one, with an uneven ſurface, both of which were of a roundiſh figure, were pulveriſed together*. 300 grains of theſe pulveriſed calculi were triturated with three ounces and a half, by meaſure, or five ounces, by weight, of lye of cauſtic ſoda. The mixture became thick, and copiouſly emitted ammoniacal gaz. After digeſtion for a night, and then boiling, with the addition of five ounces of pure water, I obtained, by filtration, five ounces of clear colourleſs liquid. Boiling water was repeatedly poured upon the ſtrainer, till what paſſed through it was almoſt taſteleſs, and remained clear, on the addition of diluted ſulphuric acid.

(*a*) The matter remaining on the ſtrainer, being dried, was an impalpable, white, taſteleſs, heavy powder, which weighed 96 grains.

(*b*) The five ounces of filtrated liquid, having been ſet apart, on ſtanding, depoſited a white, opaque, granulated, ſoap-like matter, from a colourleſs clear liquid. The liquid being decanted, the depoſit was dried, and was then an opaque, brittle, ſoap-like matter, which diſſolved readily in water, giving a clear but not viſcid ſolution, and

* The object of theſe experiments being principally to inveſtigate the properties of one of the conſtituent parts of urinary concretions, which part was previouſly determined (by the teſt of nitric acid), to exiſt in both theſe, it can be no objection to the experiments, that I made uſe of a mixture of two calculi.

 taſting

taſting weakly of ſoda. This ſoap-like matter weighed 280 grains.

(*c*) The decanted liquor, (*b*,) being mixed with the above filtrated liquors, on evaporation to three ounces, afforded no depoſit on ſtanding, although it was a very heavy and ſoapy liquid to the feel; but, on adding diluted ſulphuric acid gradually, till it ceaſed to become turbid, a ſediment was depoſited, which was a very light, white, impalpable powder, in weight, when dried, 26 grains. The liquid from which this powder was precipitated, being evaporated, afforded nothing but ſulphate of ſoda, and a few grains of cryſtals, which ſeemed to be phoſphate of ſoda. There was alſo a blackiſh matter, which burnt like horn, or other animal matter, and did not leave a pink or roſe-coloured matter, on evaporating the ſolution of it in nitric acid to dryneſs, but left a carbonaceous reſidue; whereas, the white precipitate, ſo treated, afforded a beautiful pink matter.

(*d*) 250 grains of the ſoap-like matter (*b*) being diſſolved in eight ounces of pure water;

1. A little of this ſolution, further diluted by one ounce of water, grew milky on adding a few drops of nitric acid, but became leſs ſo on ſtanding. On adding more nitric acid, and heating it, the mixture became quite clear: by adding a few drops of lye of cauſtic ſoda, a very ſlight curdy appearance took place.

2. On adding, to the ſame diluted ſolution, a little of the diluted ſulphuric or muriatic acid, milkineſs enſued, and remained, although the acids were added till the mixture was extremely ſour. On adding lye of cauſtic ſoda, much more than to ſaturate the ſuperabundant acid, the mixture became clear again; and, on adding the acids a ſecond time, the milkineſs was reproduced. It was found that the milkineſs could be produced and deſtroyed, or clearneſs be produced, by the alternate addition of the acid and alkali, for an unlimited number of times. If the nitric acid however was uſed, at length no milkineſs could be induced. If carbonate

of

of ſoda was added, in place of the cauſtic ſoda, the mixture could not be made clear.

3. Lime water was rendered turbid by this ſolution, but I neglected to examine the precipitated matter.

4. A little of the ſolution, with the addition of a few drops of concentrated nitric acid, being evaporated to dryneſs, ſometimes a pink, and at other times a blood-red, or roſe-coloured matter was left; which, by further application of fire, became black.

5. Carbonic acid, digeſted and ſhook with this ſolution, did not render it turbid.

6. To the whole of the remaining ſolution was added diluted ſulphuric acid, to ſaturate the alkali. On ſtanding, a copious precipitate took place, from a clear liquid; which precipitate, being waſhed and dried, was a maſs of very light, mica-like, whitiſh cryſtals, amounting to 123 grains. It was eſtimated that the ſolution uſed in the Experiments 1.—5. would have produced 12 grains, and that the 30 grains of ſoap-like matter, (*b*,) not decompounded, would have yielded about 14 grains more.

(*e*) The precipitate, (*d*, 6.)

1. Had no taſte, nor ſmell, and did not diſſolve in the mouth.

2. About one part of it only diſſolved in 800 parts of boiling water; which ſolution did not redden paper ſtained with turnſole, nor the ſolution and tincture of this teſt; neither did it change turnſole paper, reddened by acid, to a blue colour. On cooling, the greateſt part of what had been diſſolved was depoſited, in a cryſtalliſed ſtate, equally on the ſides and bottom of the veſſel. This cryſtalliſed matter had the properties abovementioned (*d*). Boiling water was found to diſſolve a much greater proportion of *urinary ſtone*, and alſo of *gravel*, than of this precipitate.

3. Lye of mild potaſh, or ſubcarbonate of potaſh, being dropped into the ſolution (*e*, 2.) with its cryſtalliſed depoſit, the cryſtals at firſt ſeemed to diſſolve; but, on ſtanding, a great

great part of the matter was depoſited, and the liquid remained turbid.

4. The precipitate being boiled with lye of carbonate of ſoda, more ſeemed to be diſſolved than in pure water; but the ſolution was not clear, and, on evaporating it nearly to dryneſs, and pouring cold water upon it, on a paper ſtrainer, ſcarcely any thing but the ſoda paſſed through with the water; the precipitate remaining behind on the paper. The reſult was the ſame, when this experiment was made with a lye of carbonate of ammoniac. The reſult was alſo the ſame, with water in which red oxide of mercury had been boiled; which was alſo boiled with this precipitate, and filtrated after cooling.

5. A little of the precipitate being triturated with quicklime, hot water was poured upon it. The filtrated liquor gave the precipitate back again, on adding muriatic acid.

6. The precipitate expoſed to flame, with the blowpipe, turned black, emitted the ſmell of burning animal matter, and evaporated or burnt away without any ſigns of fuſion; ſtaining the platina ſpoon black.

7. Five grains of the precipitate, in half an ounce of water, were left to ſtand in a warm room, during the months of Auguſt and September laſt, without any ſigns of putrefaction appearing, or any obvious change taking place.

8. Twenty-four ounces of boiling water were ſaturated with the precipitate, and divided into ſix portions; from each of which, on cooling, moſt of it again precipitated.

The firſt portion, on boiling with a little lye of carbonate of ſoda, (the pneumatic apparatus being affixed,) diſcharged no carbonic acid into lime water; but a tranſparent ſolution was produced, and, on cooling, very little was precipitated.

The ſecond portion was, in the ſame manner, boiled in a little lye of cauſtic ſoda; which gave a tranſparent ſolution on cooling, without any precipitation.

The

The third portion being boiled with lime water, very little more ſeemed to be diſſolved than in pure water.

The fourth portion being boiled with 4 grains of ſubphoſphate of lime, or calcined bone, no more ſeemed to be diſſolved on account of this addition.

Nor was more diſſolved in the fifth portion, by the addition of 4 grains of phoſphate of lime, made by dropping phoſphoric acid into lime water.

And the reſult was the ſame with the ſixth portion, to which were added 4 grains of ſuperphoſphate of lime, made by adding phoſphoric acid to lime water, ſo as juſt to make a clear ſolution, and then evaporating the ſolution.

9. Urine ſeemed to diſſolve, or at leaſt to ſuſpend, a greater quantity of the precipitate than mere water; ſo likewiſe did water with a little ſulphate of ſoda.

10. The precipitate did not render ſolution of hard ſoap at all curdy; but, on adding the precipitate to ſolution of ſulphuret of potaſh, it became very turbid.

11. The precipitate produced a ſtrong efferveſcence, even in the cold, with nitric acid, but the fumes were not thoſe of nitrous acid: there was a clear ſolution, which, on evaporation to dryneſs, afforded black matter, ſurrounded by a pink, or blood-red margin.

12. The ſubſtance, with ſulphuric acid, turned black, and emitted fumes copiouſly, which were ſcarcely thoſe of ſulphureous acid; and, on evaporation, a black mark only was left.

13. I firſt digeſted, and then boiled, in water, the precipitate with pruſſiate of iron; but the filtrated liquor afforded no precipitation with ſulphate of iron.

14. Two drachms, by meaſure, of nitric acid, of the ſpecific gravity of 1,35, were poured upon 7 grains of the precipitate. A violent efferveſcence took place, which was ſoon ſucceeded by a complete ſolution.

A few drops of this ſolution, being evaporated on glaſs, left

left a black mark, ſurrounded by a pink margin. A few drops of nitric acid being evaporated from this reſidue, nothing but a ſtill leſs black mark, and a few red ſpots remained.

Nitric acid being added a third time, nothing but a black mark, ſtill ſmaller, remained; which entirely diſappeared, on evaporating this acid from it a fourth time.

I found that a few drops of this ſolution, ſo diluted that they did not contain the $\frac{1}{4000}$, or even a much ſmaller part, of a grain of the precipitate, on evaporation, left a pink ſtain on glaſs.

The whole of the reſt of the ſolution was diſtilled in a very low temperature, ſo that a drop only fell about every half-minute, till a thick browniſh ſediment remained, with a red margin. A ſimilar diſtillation was performed, with the diſtilled liquor, a ſecond time, when there remained a little whitiſh thick matter. On a third diſtillation, as before, with the diſtilled liquor, towards the cloſe white fumes aroſe; and about half a drachm of liquid, which now remained in the retort, being left to ſtand, priſmatical cryſtals, decuſſating each other, were formed. They had a ſharp taſte, but were ſcarcely ſour; were very ſoluble in the mouth, and evaporated in white fumes, leaving a very ſlight black ſtain.

15. Twenty grains of the precipitate were introduced into a tube, $\frac{1}{8}$ of an inch wide in the bore, ſealed by melting at one extremity; which extremity was coated, and the tube was fitly bent for retaining ſublimate, and collecting gaz. The temperature, from the fire applied, was at firſt very low, but was gradually increaſed, ſo as to make the coated part, containing the charge, red hot. At firſt, the precipitate turned black, and a little water appeared. Secondly, gaz came over, which had the ſmell of empyreumatic *liquor cornu cervi.* Thirdly, a brown ſublimate appeared, and gaz as before, but alſo with pruſſic acid gaz. Fourthly, black matter,

matter, ſtaining the tube, as if from tar, or animal oil. On cooling, there was found a reſidue, of nearly three grains, of pure carbon. The ſublimate was principally carbonate of ammoniac; the reſt was animal oil. The gaz diſcharged was nearly half its bulk, or 5 cubic inches by meaſure, carbonic acid; and the remaining 5 cubic inches were nitrogen gaz, containing pruſſic acid and empyreumatic oil.

I treated in the ſame manner, the ſame quantity of reddiſh cryſtals, depoſited ſpontaneouſly from urine. The reſult was not very different from that of the former experiment. The gaz was more offenſive, ſmelling like putrid urine, and the carbonaceous reſidue was more copious, and contained lime and phoſphoric acid; at leaſt the lixivium of it became white, on dropping into it oxalic acid; and it became ſlightly curdy, on adding lime water.

I treated in the ſame manner, ſome quite round and ſmooth concretions, of the ſize of black pepper ſeeds. The products were the ſame as the former, but the gaz was ſtill more offenſive, and in ſmaller quantity; and the carbonaceous matter was more copious.

I, in the ſame way, ſubjected to experiment 20 grains of a nut-brown light calculus, which I had previouſly aſcertained to contain the matter above deſcribed, which was precipitated from cauſtic ſoda by acids. The products were of the ſame kind as the former; but I could find no trace of phoſphoric acid in the reſidue, which I did of lime, and the gaz was leſs offenſive. The carbonaceous reſidue was not, in weight, 3 grains.

It will be proper, before I proceed further, to point out ſome of the more obvious concluſions from the above experiments.

1. It appears that at leaſt one half of the matter of the urinary concretions ſubjected to the above experiments united to cauſtic ſoda, and was precipitated from it by acids. (II. *a*—*d*.)

2. This

2. This precipitate does not indicate acidity to the moſt delicate teſts; (*e*, 2.) and, as it is inodorous, taſteleſs, (*e*, 1.) ſcarcely ſoluble in cold water, (*e*, 2.) does not unite to the alkali of carbonate of potaſh, of ſoda, or of ammoniac, (*e*, 3, 4.) nor to oxyde of mercury, (*e*, 4.) nor to the lime of lime water, (*e*, 8.) nor decompound ſoap, (*e*, 10.) or pruſſiate of iron, (*e*, 13.) and, as its combination with cauſtic ſoda reſembles ſoap, more than any double ſalt known to conſiſt of an acid and alkali, this precipitate does not belong to the genus *acids*.

3. As this precipitate could not be ſublimed, without being decompounded, like animal matter, (*e*, 15.) and alſo for the reaſons mentioned in the laſt paragraph, it cannot be the ſame thing as the *acid ſublimate* of Scheele, or the ſuccinic acid.

4. As it does not appear to be putreſcible, nor form a viſcid ſolution with water, it cannot be referred to the *animal mucilages*.

5. On account of its manner of burning in the air, under the blowpipe, (*e*, 6.) and its yielding, on expoſure to fire in cloſe veſſels, the diſtinguiſhing products of animal matter, (eſpecially ammoniac and pruſſic acid,) as well as on account of its affording a ſoap-like matter with cauſtic ſoda, this precipitate may be conſidered as a ſpecies of animal matter; and, from its compoſition being analogous to that of the ſubſtances called, in the new ſyſtem of chemiſtry, *animal oxides*, it belongs to that genus. Its peculiar and ſpecific diſtinguiſhing properties are, *imputreſcibility*, *facility of cryſtalliſation*, *inſolubility in cold water*, and, that moſt remarkable property of all others, *producing a pink or red matter, on evaporation of its ſolution in nitric acid**.

I do

* It is much to be wiſhed that we poſſeſſed equally delicate teſts of the other ſpecies of animal matter, which are confounded together, although, from their obvious properties, there is reaſon to believe they are of very

I do not avail myſelf of various other concluſions in this place, becauſe they relate eſpecially to the agency of medicines for preventing and removing concretions; and of courſe do not properly fall within the views of the Royal Society.

Having found the above precipitate to be an oxide, and not, as is commonly ſuppoſed, an acid, I thought it probable that, like other analogous oxides, it was *acidifiable*, and I ſuſpected that I had really rendered it into the acid ſtate, by the nitric acid; which, in the above experiments, (*e*, 14.) had imparted oxygen to it, and thereby rendered it ſoluble, deliqueſcent, pungent, and volatile. This change alſo would account for the nitric ſolution not affording the precipitate.

In order to obtain, for examination, an adequate quantity of this ſuppoſed acid, the following experiments were inſtituted, with the three acids (*viz.* the oxymuriatic, the nitro-muriatic, and the nitric), which can acidify oxides analogous to the preſent one.

Experiment 1. Twenty-five grains of the above animal oxide, (for ſo I will now venture to call it,) and three ounces of nitric acid, of the ſpecific gravity of 1,25, were put into a retort, and the hydro-pneumatic apparatus was adjoined.

At a very low temperature, a clear ſolution was made. Firſt, ſoon after the ſolution began to boil, 23 ounces, by meaſure, of colourleſs gaz came over, which were ſucceeded (ſecondly), by white fumes, filling the apparatus, and 23 ounces more of gaz. Thirdly, a white ſublimate aſcended, and there was a ſtrong ſmell of pruſſic acid. The ſublimate was very readily waſhed out, being very ſoluble, and taſted pungent or ſharp, but not ſour. Fourthly, the diſtillation being renewed, more white ſublimate appeared, but only 3

different kinds, as is the caſe with the matter of the brain, liver, voluntary muſcles, mucus, &c. Mr. Hunter has diſcovered a diſtinguiſhing ſpecific property of pus, and one is here indicated for the oxide of urinary concretions.

ounces

ounces more of gaz came over; and then the retort only contained a dark-brown ſolid matter.

The firſt portion of gaz, *viz.* 23 ounces, conſiſted of about equal bulks of carbonic acid and atmoſpherical air. The ſecond portion, *viz.* 23 ounces, was two-thirds of its bulk carbonic acid, and the reſt nitrogen gaz. The third portion, or 3 ounces, was atmoſpherical air, with a little carbonic acid.

Nitric acid was poured, in the ſame quantity as before, into the retort. An efferveſcence immediately took place, which was ſucceeded by a tranſparent ſolution. The diſtillation yielded gaz of the ſame kind as before, but in ſmaller quantity, with white fumes, and white ſublimate. When only about 4 drachms, by meaſure, of liquid remained in the retort, a little of it was evaporated; and, when reduced to a ſolid matter, it turned black, and took fire, leaving a carbonaceous reſidue; but, before this, a margin of beautiful pink matter appeared.

Nitric acid was poured, as before, into the retort, for the third time, but very little gaz aſcended, and much leſs white fumes than before. The diſtillation proceeded, till about one drachm-meaſure of liquid remained in the retort: this being left to ſtand, priſmatic cryſtals were formed in a very ſmall quantity of liquid. Theſe cryſtals did not taſte ſour, but ſharp, and they reddened turnſole paper. Adding a little ſoda to a part of them, to ſee whether I could form a neutral ſalt, I was ſurpriſed by the extrication of ammoniac. To another portion of cryſtals I added ſulphuric acid, which diſengaged nitric acid. A third portion of cryſtals, being expoſed over a lamp, wholly evaporated, without leaving a mark behind. The remaining matter in the retort being examined, was found to be nitrate of ammoniac. It was plain that the nitric acid had, by parting with oxygen to the carbon of the oxide, formed carbonic acid. The carbon being thus carried off, of courſe the nitrogen and hydrogen of the oxide uniting produce ammoniac; which, uniting with

the redundant nitric acid, compoſes nitrate of ammoniac; but great part of the nitrate of ammoniac was carried off in the vapour ſtate, exhibiting white fumes, and ſublimate, as above obſerved.

The mode of making the experiments with the other acids was of courſe different from the former experiment.

Experiment II. Twenty-five grains of the above animal oxide, and half an ounce of water, were put into a bottle capable of containing three pints; a ſtream of oxymuriatic acid gaz, from manganeſe and muriatic acid, was made to paſs into the bottle, and upon the charge, for twelve hours; and, for twenty-four hours more, oxymuriatic gaz kept iſſuing, but in ſmaller quantity, and circulating through the bottle. The oxide, by this time, was completely diſſolved. Upon adding lime to a little of the ſolution of it, ammoniac was diſengaged; and, upon adding ſulphuric acid, there was a diſengagement of oxymuriatic acid. On evaporation, however, I obtained nothing but muriate of ammoniac, with which was mixed a little manganeſe.

In this experiment, I could not doubt that the carbon had been carried off, in the ſtate of carbonic acid, by the oxygen of the oxymuriatic acid; and thus ammoniac was compounded, from the union of the two remaining conſtituent parts of the oxide, *viz.* the nitrogen and hydrogen. The oxymuriatic acid, united to the ammoniac, parted with oxygen, and became muriatic acid during evaporation; hence, muriate of ammoniac was formed.

Experiment III. The above experiment was repeated, only the gaz was nitro-muriatic gaz, from a mixture of nitric and muriatic acids. The reſult was the ſame as in the laſt experiment, except that the product was a mixture of nitrate, and muriate, of ammoniac.

I made other experiments of the ſame kind; but their reſults were ſo nearly the ſame as thoſe above related, that I ſhall not give an account of them. By the unexpected iſſue of theſe experiments, all my hopes of acidifying the

animal

animal oxide were exploded; but I am indebted to that purſuit for the curious diſcovery of the change of the moſt common baſis of urinary concretions, (the animal oxide,) into ammoniac and carbonic acid, by the oxygen of the above acids; which will be found extremely important, as it enables us to interpret many phænomena, in a variety of caſes beſide the preſent. It now appears, that the inflammation mentioned in one of the above experiments, (and which alſo happened in ſeveral others,) on evaporation of the nitric ſolution of the animal oxide, was from the nitrate of ammoniac, the *nitrum flammans* of the old chemiſts, compounded in thoſe experiments. This inflammation takes place ſometimes, on evaporation of nitric ſolutions, both of urinary concretions, and of urine itſelf evaporated to the ſtate of ſoft extract, on account of the ammoniac already exiſting in theſe ſubſtances. The compoſition of ammoniac alſo explains the diſappearance of the whole matter of ſome ſorts of urinary concretions, a very ſmall reſidue of black matter excepted, by repeated affuſion and evaporation of nitric acid, from the ſolution of them in this menſtruum.

It remains for me to give an account of the 96 grains of powdery matter left on the paper ſtrainer, (*a*;) which are the inſoluble portion, in lye of cauſtic ſoda, of 300 grains of urinary concretions.

1. A ſmall portion of the inſoluble matter, being expoſed to flame with the blowpipe, did not turn black, nor yield any ſmell of animal matter; but it became whiter, and I could juſt agglutinate the powder into one maſs, although I was unable to render it fluid.

2. The filtrated liquid, from a little of the matter boiled in water, became very turbid and white with oxalic acid: with lime water it grew barely curdy; and it did not alter the colour of turnſole, or of violet juice.

3. The matter diſſolved completely in muriatic acid, and alſo in nitric acid, without effervescence.

This nitric ſolution, having been evaporated, to carry off moſt of the free acid, inſtantly became very curdy on the addition of lime water.

It grew thick and white on adding ſulphuric acid, yielding a copious precipitate of ſulphate of lime. One portion of the ſupernatant liquor upon this precipitate, on evaporation, afforded an extract-like matter; which readily melted, as phoſphoric acid does when it is mixed with a little earthy matter. To the other portion of this ſupernatant liquor was added liquid cauſtic ammoniac, producing a precipitate which afforded no ſulphate of magneſia with ſulphuric acid.

From theſe experiments it appears, that the above 96 grains of inſoluble matter conſiſted of phoſphate of lime. Accordingly, the 300 grains of urinary concretions examined, appear to contain,

	grains
Peculiar animal oxide - - -	175
Phoſphate of lime - - -	96
Ammoniac, (and moſt probably phoſphoric acid united to the ammoniac,) water, and common mucilage of urine, which were not collected and weighed, by eſtimation - - -	29
	300

[To be continued.]

IX. *Chemical Conſiderations on the Uſe of the Oxyds of Iron in dyeing Cotton. By J. A. Chaptal. From the* Annales de Chimie, *Vol. XXVI.*

THE oxyd of iron has ſuch an affinity with cotton thread, that, if the latter be plunged in a ſaturated ſolution of iron in any acid whatever, it immediately aſſumes a chamoy yellow colour, more or leſs dark according to the ſtrength of the liquors.

liquors. It is both a curious and eaſy experiment, that when cotton is made to paſs through a ſolution of the ſulphat of iron, rendered turbid by the oxyd which remains ſuſpended in the liquor, it will be ſufficient to dip the cotton in the bath to catch the laſt particle of the oxyd, and to reſtore to the liquor the tranſparency it has loſt. The ſolution then, which before had a yellowiſh appearance, becomes more or leſs green, according as it is more or leſs charged.

The colour given to cotton by the oxyd of iron becomes darker, merely by expoſure to the air; and this colour, ſoft and agreeable when taken from the bath, becomes harſh and ochry by the progreſſive oxydation of the metal. The colour of the oxyd of iron is very faſt: it reſiſts not only the air and water, but alſo alcaline leys, and ſoap gives it ſplendour without ſenſibly diminiſhing its intenſity. It is on account of theſe properties that the oxyd of iron has been introduced into the art of dyeing, and been made a colouring principle of the utmoſt value. But I have been able to give a new extenſion to the applications of this oxyd; and I ſhall here confine myſelf to the only reſults I have obtained worthy of entering into the operations of the dye-houſe, and which have been employed with ſucceſs for ſeveral years in my manufactory.

In order that the oxyd of iron may be conveniently applied to the cotton thread, it is neceſſary to begin by effecting its ſolution, and in this caſe acids are employed as the moſt uſeful ſolvents. Dyers almoſt every where make a myſtery of the acid which they employ; but it is always the acetous, the ſulphuric, the nitric, or the muriatic. Some of them aſcribe great differences to the ſolution of iron by the one or the other acid, but in general they give the preference to the acetous. This predilection appears to me to be founded much leſs on the difference of the colours that may be communicated by the one or the other ſalt, than on the different degrees of corroſive power which each exerciſes

on the ſtuff. That of the ſulphat and muriat is ſo great, that, if the ſtuff be not waſhed when it comes from the bath, it will certainly be burnt; whereas ſolutions by the acetous or any other vegetable acid are not attended with the like inconvenience.

Iron appears to be at the ſame degree of oxydation in the different acids, ſince it produces the ſame ſhade of colour when precipitated; and any acid ſolvent may be employed indiſcriminately, provided the nature of the ſalt and the degree of the ſaturation of the acid be ſufficiently known; for the ſubſequent operations may be then directed according to this knowledge, and the inconveniences which attend the uſe of ſome of theſe ſalts may be prevented. This without doubt is a great advantage which the man of ſcience enjoys over the mere workman, who is incapable of varying his proceſs according to the nature and ſtate of the ſalts which he employs.

In this paper I ſhall confine myſelf to making known the colour that may be obtained from the oxyd of iron, 1ſt, employed alone on ſtuff which has received no previous preparation; 2d, employed together with madder on ſtuff prepared to receive the Adrianople red.

1. If the ſulphat of iron or any other martial ſalt be diſſolved in water, and cotton be dipped in the liquid, the cotton will aſſume a chamoy colour, more or leſs dark according as the ſolution is more or leſs charged. The affinity of the cotton to the iron is ſo great, that it attracts the metal, and takes it in a great meaſure from the acid by which it was diſſolved.

2. If the iron of a pretty ſtrong ſolution be precipitated by an alcaline liquor that ſhews five or ſix degrees (by the areometer of Baumé), the reſult will be a greeniſh blue magma. The cotton macerated in this precipitate aſſumes at firſt an unequal tint of dirty green; but mere expoſure to the air makes it in a little time turn yellow, and the ſhade is very dark.

It is by ſuch or almoſt ſimilar proceſſes that dyers communicate, what is called among workmen, an *ochre* or *ruſt colour*. But theſe colours are attended with ſeveral inconveniences to the artiſt. 1. Strong ſhades burn or injure the cloth. 2. This colour is harſh, diſagreeable to the eye, and cannot be eaſily united with the mild colours furniſhed by vegetables. I made attempts therefore to avoid theſe inconveniences, and ſucceeded in the following manner: I tread the cotton cold in a ſolution of the ſulphat of iron, marking three degrees, wring it carefully, and immediately plunge it in a ley of potaſh at two degrees, upon which I have poured to ſaturation a ſolution of the ſulphat of alumine: the colour is then brightened, and becomes infinitely more delicate, ſoft, and agreeable. The ſulphat no longer attacks the tiſſue of the ſtuff; and after the cotton has been left in the bath for four or five hours, it is taken out to be wrung, waſhen and dried. In this manner we may obtain every ſhade that can be wiſhed, by graduating the ſtrength of the ſolutions. This ſimple proceſs, the theory of which preſents itſelf to the mind of every chemiſt, has the advantage of furniſhing a colour very agreeable, exceedingly fixed, and, above all, extremely economical. I employ it with great advantage in dying nankins, the colour of which has infinitely more fixity than that of the Engliſh (dyed) nankins*. It poſſeſſes over the latter the advantage of reſiſting leys; and the only fault I have diſcovered in it is, that it becomes brown by the action of aſtringents.

I thought for ſome time that it would be poſſible to combine this yellow with the blue of indigo, to obtain a durable green: but hitherto I have been deceived in my hopes; and it reſults from different trials which I made on this ſubject, that there is not a ſufficient affinity between the blue of

* Was the author aware that the Engliſh nankins are made from a cotton wool of the natural colour that requires no dyeing proceſs? Some jeans and other ſtuffs are dyed by means of oxyd of iron: to theſe, perhaps, he alludes. EDIT.

indigo

indigo and the oxyds of iron. I obtained only a dirty earthy green of different ſhades, and exceedingly poor. The oxyd of iron combines on the other hand very eaſily with the red of madder, and produces a bright violet or plum (*pruneau*) colour, the uſe of which is as extenſive as beneficial in the cotton manufactory. But if we ſhould confine ourſelves to apply theſe two colours to cotton, without having employed a mordant capable of fixing the latter, the colour would not only remain dull and diſagreeable by the impoſſibility of brightening it, but it would ſtill be attended with the great inconvenience of not reſiſting leys. We muſt begin then by preparing the cotton, as if to diſpoſe it for receiving the Adrianople red; and when it has been brought to the operation of galling, it is to be paſſed through a ſolution of iron, more or leſs charged according to the nature of the violet required: it is then to be carefully waſhed, twice maddered, and brightened in a bath of ſoap.

When a real velvety rich violet is required, it is not to be paſſed through the ſolution of iron till it has been previouſly galled; the iron is then precipitated in a blueiſh oxyd, which combined with the red of madder gives a moſt brilliant purple, more or leſs dark according to the ſtrength of the galling and of the ferruginous ſolution. It is very difficult to obtain an equal colour by this proceſs, and in manufactories an equal violet is conſidered as a maſter-piece of art. It is generally believed that it is only by well-directed manipulations that it is poſſible to reſolve this problem, of ſo much importance in dyeing. But I am convinced that the great cauſe of the inequality in this dye is, that the iron depoſited on the cotton receives an oxydation merely by expoſure to the air, which varies in different parts of it. The threads which are on the outſide of the hank are ſtrongly oxydated, while thoſe in the inſide, removed from the action of the air, experience no change. It thence follows, that the inſide of the hank preſents a weak ſhade, while the

exterior

exterior part exhibits a violet almoſt black. The means to remedy this inconvenience is, to waſh the cotton when it is taken from the ſolution of iron, and to expoſe it to the madder moiſt. The colour will become more equal and velvety. The ſolvents of iron are almoſt the ſame for this colour as for the yellow colour already mentioned.

I ſuppreſs here every thing that regards manipulation, that I may attend only to chemical relations; and on that account I ſhall mention an obſervation which may ſerve to guide the artiſt in brightening the violet on his cotton. The red of madder and the oxyd of iron depoſited on the ſtuff determine the violet colour. This colour becomes red or blue, according as either of the two principles predominates. The dyer knows by experience how difficult it is to obtain a combination which produces the tone of colour deſired, eſpecially when it is required to be very full, lively and durable. This object, however, may be obtained, not only by varying the proportions of the two colouring principles, but alſo by varying the proceſs of brightening. The only point is, to be acquainted with the two following facts; that the ſoda deſtroys the iron, while the ſoap, by ſtrong ebullition, ſeizes in preference the red of the madder. Hence it is, that the colour may be inclined to red or blue, according as you brighten with one or the other of theſe mordants. Thus, cotton taken from the madder dye, when waſhen and boiled in the brightening liquor with 30,00 of ſoap, will give a ſuperb violet; whereas you will obtain only a plum colour in treating it with ſoda.

The oxyd of iron precipitated on any ſtuff unites alſo very advantageouſly with the fawn colour furniſhed by aſtringents, and by varying the ſtrength of mordants an infinity of ſhades may be produced. In this caſe, it is leſs a combination or ſolution of principles, than the ſimple mixture or juxta-poſition of the colouring bodies on the ſtuff. By means of a boiling heat, we may combine in a more intimate manner the oxyd of iron with the aſtringent principle; and

then

then it is brought to the ſtate of black oxyd, as has been obſerved by our colleague Berthollet. It is poſſible alſo to embrown theſe colours, and to give them a variety of tints, from the bright grey to the deep black, by merely paſſing the cottons impregnated with the aſtringent principle through a ſolution of iron. The oxyd is then precipitated itſelf by the principle which is fixed on the ſtuff.

An obſervation, which may become of the utmoſt value for the art of dyeing, is, that the moſt uſual aſtringent vegetables all furniſh a yellow colour, which has not much brilliancy, but which has ſufficient fixity to be employed with advantage. This yellow colour is brightened in the ſeries of vegetables, in proportion as the aſtringent principle is diminiſhed, and the vivacity of the colour is augmented in the ſame proportion. It is difficult then to obtain yellow colours which are at the ſame time durable and brilliant. Theſe two valuable qualities are to each other in an inverſe ratio; but it is poſſible to unite the colouring principles in ſuch a manner as to combine ſplendour with fixity. Green oak bark unites perfectly with yellow weed, and ſumach with green citron. It is by this mixture that we may be able to combine with the oxyd of iron vegetable colours, the ſplendour of which is equal to their durability.

I ſhall conclude theſe reflections with an obſervation in regard to the employment of aſtringents in the dyeing of cotton. It has been pretended, that by increaſing the proportions of ſumach and the bark of alder tree or oak, theſe ſubſtances might ſupply the place of gall-nuts in dyeing cotton red. I ſhould have received the more pleaſure from this diſcovery, as galls tend to render our colours conſiderably dearer, and as I could have procured ſumach at a very low price, ſince it grows almoſt every where in the dry parts of our ſouthern diſtricts. But I can ſafely aſſert, that it is impoſſible to employ theſe as ſubſtitutes, in whatever doſes they may be uſed. The colour is always much paler, poorer, and leſs fixed. I know that the caſe is not the ſame in re-

gard

gard to dyeing wool and silk, in which it may be employed with success; and in giving an account of this difference, I think the cause of it may be found in the nature of the gall-nuts. 1. The acid which they exclusively contain, as Berthollet has proved, facilitates the decomposition of the soap with which the cottons have been impregnated, and the oil then remains fixed in their tissue, and in a greater quantity, as well as in a more intimate combination. 2. The gall-nuts, which owe their development to animal bodies, retain a character of animalisation, which they transmit to the vegetable stuff, and by these means augment its affinities with the colouring principle of the madder; for it is well known of what utility animal substances are to facilitate this combination. This animalisation becomes useless in operating upon woollen or silk.

X. *Description of a Thermometer which marks the greatest Degrees of Heat and Cold, from one Time of Observation to another, and may also register its own Height at every Instant. By* ALEX. KEITH, *Esq. F.R.S. and F.A.S. Edin. From the* Transactions of the Royal Society of Edinburgh, *Vol. IV.* 1798.

THERMOMETERS have hitherto been defective for meteorological purposes, in so far as they only point out the degree of heat at the moment of inspecting them, but do not shew what the difference has been, from the time of one observation to another.

The ingenious Robert Hook, in the end of the last century, mentions his intention of making a thermometer for the above purpose; but it does not appear that it was ever executed: neither does he explain how it was to have been done.

The thermometer, invented by Mr. James Six, as described in the 72d volume of the Philosophical Transactions of the Royal

Royal Society of London, is made to shew its greatest rise or fall from one period of observation to another. This is done by means of two small pieces of black glass, which float on two different surfaces of mercury, within two glass tubes hermetically sealed. These floats, when raised to their greatest height, adhere to the sides of the tube, by means of a spring of glass, and become stationary, although the mercury falls. After the observer has noted the temperature, he, by a magnet held in his hand, draws down the float to the surface of the mercury, in consequence of a small bit of steel wire being inclosed in the float, and the instrument is prepared for another observation. This is an ingenious invention, but requires too delicate workmanship to be fit for common use: besides, it cannot be made to record the degrees of heat at intermediate periods.

The thermometer lately invented by Dr. Rutherford, of Balliliſh, and described in the 3d vol. of the Edinburgh Transactions, is also an ingenious contrivance, but has the same defect of marking only the extreme points to which the liquor has risen or fallen in two separate glass tubes.

Some years ago it occurred to me, that an air thermometer might be used for the purposes required; and accordingly I read to this society a description of the instrument. But having formed another instrument of a more simple construction, I beg leave to give a description of it.

A B, fig. 1. (Plate I.) is a tube about 14 inches long, and $\frac{3}{4}$ of an inch calibre, of thin glass, sealed or closed at top. To the bottom, which is bent upwards, there is joined a glass tube 7 inches long, and $\frac{1}{10}$ths of an inch calibre, open at top. The tube A B is filled with alcohol, and from B to E is filled with mercury.—As liquids are incompressible by weight in any perceptible degree, neither the alcohol nor mercury will be affected by the weight of the atmosphere.

F D is a scale of brass or ivory, about $6\frac{1}{2}$ inches long, divided in the usual way. E is a small conical piece of ivory or glass, of a proper weight, made to float on the surface of the

the mercury in the ſmaller tube; to which float is joined a wire, termed the float-wire, reaching to H, having a knee bent at a right angle, which raiſes one index, and depreſſes another, according as the mercury riſes or falls. This part of the apparatus is defended from wind or rain by means of a glaſs tube $7\frac{1}{2}$ inches long, cloſed at top and open at bottom, ſo wide as to ſlide eaſily over the ſcale, and, by means of a braſs rim cemented to it, made to fit exactly to the circular baſe of the ſcale. This cover need not be taken off, except when the inſtrument is to be prepared for an obſervation. [The operation of the float and indexes will be better underſtood from fig. 4, where a ſimilar apparatus, but belonging to a newly invented barometer, is repreſented on a larger ſcale.]

F G is the ſcale fixed to a circular piece of wood or braſs D, through which the ſmall tube is made to paſs. From G to F is a piece of the ſmalleſt gold wire ſtretched along the ſcale, fixed at the ends by two pins. L L are two indexes, formed of thin black oiled ſilk, pierced by the ſmall wire in ſuch a manner as to ſlide upwards and downwards with a very ſmall force, not more than two grains. H, the knee of the float-wire before deſcribed, is made to encompaſs the ſmall wire between the two indexes, ſo that, when the float riſes, the upper index is moved upwards, and, when it deſcends, it leaves the upper index ſtationary, and puſhes down the lower one, which is alſo left ſtationary when the float riſes. When the inſtrument is to be prepared for an obſervation, the one index is to be pulled down, and the other raiſed, by means of a bit of wire, bent for the purpoſe, until both of them touch the knee of the float-wire: and when it is again obſerved, the upper index will point out the greateſt degree of heat, and the lower the greateſt degree of cold, ſince the time they were ſet.

If this thermometer is to be adapted to a piece of clock-work, in order to record the degrees of heat at each hour and minute of time, it ought to be made of larger dimen-

ſions.

ſions. The large tube may be 40 inches long, and not increaſed in diameter; but the ſmall tube ought to be enlarged in diameter, and not in length. The alcohol will thus be affected by heat or cold in as ſhort a time as that before deſcribed.

It is unneceſſary to explain the clock-work. It is ſufficient to ſay, that a hollow cylinder of any light ſubſtance, 7 inches long and 5 inches diameter, is made to revolve upon a vertical axis once in 31 days or a month; a piece of ſmooth vellum or paper is put round this cylinder, paſted only at the joining, but ſo as to make it adhere cloſe to the cylinder: on this are drawn 31 equal perpendicular diviſions, numbered at top, 1, 2, 3, &c. each of which is ſubdivided into ſix parts, to anſwer to 4 hours. The length of this cylinder is divided by lines ſurrounding it, or zones, in ſuch number as to correſpond to the ſcale of Fahrenheit's therm. viz. from 0 to 100°. Theſe diviſions ought to be engraved on copper, and a number of impreſſions thrown off on ſmooth or vellum paper, in order that one may be ready to put on each month.

M N, fig. 2. repreſents the cylinder covered with one of theſe impreſſions. P P is the ſcale fixed to the frame in which the cylinder turns. The diviſions on this ſcale correſpond to thoſe on the cylinder.

Q is a piece of black lead pencil, joined to the end of the float-wire, in place of the knee before mentioned. This pencil is made to preſs lightly on the cylinder, by means of the ſmall weight R. And as the pencil riſes or falls by heat and cold, it marks the degrees on the ſcale of the cylinder; and the cylinder being conſtantly revolving, the diviſion for each day and parts of a day will ſucceſſively be marked by the pencil, which will leave a trace, deſcribing an undulated line, diſtinctly delineating the temperature of each day through the month. Theſe papers, when taken off and bound together, will make a complete regiſter of the temperature for the year; or, if they are paſted to one another,

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in their order, they will form a thermometrical chart, by which the variations of heat and cold, during the year, may all be ſeen and compared by one glance of the eye.

By inſpecting fig. 2. the effect of the inſtrument may be ſeen. It appears that the paper had been put on the cylinder the firſt day of the month, at mid-day, when the thermometer ſtood at 45°; that it fell gradually till mid-night to 25°; thereafter it roſe till the 2d, at 1 P. M. when it ſtood at 42°; then it deſcended at mid-night to 35°; that on the 4th at noon it roſe to 50; and at noon the 10th of the month it ſtands at 40°.

If 3 inches be added to the length of the cylinder, it may be made to delineate the variations of the barometer as well as the thermometer, and thereby to form a complete chart or view of the progreſs of both of them. And if inſtruments of this kind were kept in different parts of the country, and their charts compared together, it would afford much information with regard to meteorology.

XI. *Deſcription of a Barometer, which marks the Riſe and Fall of the Mercury from two different Times of Obſervation; invented by* ALEX. KEITH, *Eſq. F. R. S. and F. A. S. Edin. From the* Tranſactions of the Royal Society of Edinburgh, *Vol. IV.* 1798.

ABCD, fig. 3. (Plate I.) is a glaſs tube bent in the manner repreſented, open at D, and hermetically ſealed at A. From A to B is 8 inches long, and about $\frac{3}{4}$ of an inch calibre. From B to C 31$\frac{1}{2}$ inches long, and about $\frac{1}{4}$ inch calibre. And from C to D 4$\frac{1}{2}$ inches long, and $\frac{1}{2}$ inch calibre. The tube is filled with mercury, the length from A to E being 29$\frac{1}{2}$ inches. When the tube is hung perpendicularly, the mercury will fall from B towards E, leaving a vacuum from A to B. When the atmoſphere becomes hea-

vier, the mercury falls in the tube D C, and when lighter it rises. The range of the scale is about 3 inches, being equal to that of a common barometer of the best construction, which has a bason with a very broad surface. This instrument moves in a direction contrary to that of the common barometer, the one rising while the other falls.

Fig. 4 represents the tube D C, &c. on a larger scale. This part of the instrument is constructed exactly in the same way as the scale, float, &c. of the thermometer described in the article immediately preceding the present. F is the float, having the float-wire fixed to it, terminating in a knee at a right angle between the indexes L L, where it embraces a very small wire stretched along the scale, and thereby raises or lowers them as the mercury rises or falls in the tube D C.

The barometer is prepared for observation, by bringing down the one and raising the other index till both touch the knee of the float-wire. When next observed, the upper index will point out the greatest depression of the mercury —or lightness of the atmosphere; and the lower the greatest rise of the mercury—or weight of the atmosphere since the scale was prepared.

By this means, the variations of the atmosphere are much more truly pointed out than by the common barometer; for it often happens, that, during tempestuous weather, or before it, the mercury both rises and falls within a few hours, or during the night-time; which variations cannot be noticed by any of the barometers now in use. The sudden fall and rise, or even the rise and fall of the mercury, always denote an extraordinary agitation in the atmosphere. By a common barometer the mercury may be at the same height in the morning that it was the night before; which leads to a conclusion, that as there has been no agitation of the mercury, there will be calm or settled weather: but this new barometer will often shew in such cases, that the

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one float has been raiſed $\frac{2}{10}$, and the other depreſſed as much; which, inſtead of indicating calm weather, denotes that tempeſtuous weather may be expected.

The weight of the atmoſphere at great heights might be diſcovered, by ſuſpending this inſtrument to an air-balloon.

XII. *On the different Properties of Metal and Wooden Springs.*

THE ſpring is, in a variety of pieces of mechaniſm, not only a very uſeful auxiliary, but in ſome (as in pocket-watches, gun-locks, &c.) an indiſpenſible requiſite. But from the difficulty of getting ſprings to ſtand, as the workmen expreſs it, to their temper, they are not ſo frequently applied to the purpoſes of mechanics as otherwiſe they might be. Great judgment and ſkill, as every one knows who is converſant with the ſubject, are required to give to a metal ſpring its due degree of temper; for, if made too hard, it ſnaps; if not hard enough, it ſets. Metal ſprings, however, frequently fail from another cauſe, which is very little underſtood; in conſequence of which, the failure is uſually attributed, though, as preſently will be ſeen, unjuſtly, to the unſkilfulneſs of the workman. It is a circumſtance not commonly obſerved reſpecting a metal ſpring, that if it has not ſomething to ſtop againſt, but is ſuffered to vibrate after performing the requiſite action, it will, in a ſhort ſpace of time, if the action be frequently repeated, either break or ſet. Whence this property ariſes is not at preſent the object of enquiry. It is mentioned that, in caſes which will admit of it, this inconveniency may be guarded againſt. In thoſe caſes in which the vibration cannot conveniently be avoided, a wooden ſpring, which is not ſubject to the like inconveniency, is the beſt, and, perhaps, the only ſubſtitute. A wooden ſpring is, in the property alluded to, the reverſe of a metal one: if ſtopped in its vibration, it ſoon ſets or breaks;

if permitted to vibrate, its temper or elaſticity ſuffers not the ſmalleſt diminution.

A gentleman, ſome few years ago, eſtabliſhed a mill for a manufacturing purpoſe, in which, having occaſion to apply ſprings under the circumſtances mentioned above, he at firſt attempted to make uſe of metal ones, but in vain, being never able to make them ſtand a ſingle day's work. He tried every kind of ſteel, and employed many different workmen, but ſtill without ſucceſs. Merely as a temporary expedient, till ſuch time as he could get a freſh ſupply of ſteel ſprings, he one day tried a wooden one, which, to his agreeable ſurpriſe, completely anſwered his purpoſe; and from that time, as may be concluded, he never uſed any other than wooden ones. The experiment was perfectly deciſive: the ſprings were in daily action for four years ſucceſſively, making, in a common way, from forty to fifty ſtrokes in a minute on an average. At the expiration of the four years, thoſe ſprings which had eſcaped accidents were as elaſtic, and as ſtrong, as when firſt put into action. The wood they were made of was red deal, clean grained, and perfectly free from knots.

To many manufacturers who employ machinery for various purpoſes, in which ſprings, that muſt be ſuffered to vibrate, form a part, this information, which may to ſome appear trivial, will be found highly uſeful.

XIII. *The Proceſs ſuggeſted by* JAMES CARMICHAEL SMYTH, *M.D. F.R.S. Fellow of the Royal College of Phyſicians, and Phyſician Extraordinary to his Majeſty, for determining the Effect of the Nitric Acid in deſtroying Contagion. Extracted from* An Account of the Experiment made at the Deſire of the Lords Commiſſioners of the Admiralty on board the Union Hoſpital Ship. 1796.

THE ſubject of this article is of the greateſt importance, and the facts which have been eſtabliſhed by the experiments

ments made on board the Union and other ſhips at Sheerneſs, are worthy of attention, and cannot be too ſtrongly recommended to the notice of every perſon who wiſhes to be maſter of a ſimple and efficacious method for deſtroying contagion, or preventing it from breaking out, where there may be reaſon to fear, from concurring circumſtances, that there is a danger of ſuch an evil manifeſting itſelf.

The experiment on board the Union was conducted by Mr. Menzies, late ſurgeon to his majeſty's ſloop Diſcovery, and by Mr. Baſſan, ſurgeon of the Union; and when it is conſidered, that freſh contagion was daily pouring into the hoſpital from the Ruſſian veſſels, which were at that time lying in the Downs, and which had brought with them a ſpecies of fever that might in every ſenſe of the word be termed an epidemy, it will be allowed, that the ſucceſs which attended it was ſuch that it cannot be too generally known.

The wards were extremely crowded, and the ſick of every deſcription lay in cradles, promiſcuouſly arranged, to the number of nearly two hundred; of which, about one hundred and fifty were in different ſtages of the above malignant fever, which was extremely contagious, as appeared evident from its rapid progreſs and fatal effects, among the attendants on the ſick, and the ſhip's company.

The utenſils and materials provided for the proceſs were the following: A quantity of fine ſand, about two dozen quart earthen pipkins, as many common tea-cups, ſome long ſlips of glaſs to be uſed as ſpatulas, a quantity of concentrated vitriolic (ſulphuric) acid, and a quantity of pure nitre (nitrat of potaſh).

The proceſs was conducted in the following manner: 1ſt, All the ports and ſcuttles were ſhut up; the ſand, which had been previouſly heated in iron pots, was then ſcooped out into the pipkins by means of an iron ladle; and in this heated ſand, in each pipkin, a ſmall tea-cup was immerſed, containing about half an ounce of the ſulphuric acid, to which, after it had acquired a proper degree of heat, an equal quantity of nitrat of potaſh in powder was gradually

added, and the mixture ſtirred with a glaſs ſpatula till the vapour aroſe from it in conſiderable quantity *. The pipkins were then carried through the wards by the nurſes and convaleſcents, who kept walking about with them in their hands, occaſionally putting them under the cradles of the ſick, and in every corner where any foul air was ſuſpected to lodge. In this manner they continued fumigating, until the whole ſpace between decks was fore and aft filled with the vapour, which appeared like a thick haze.

The vapour at firſt excited a good deal of coughing among the patients, which gradually ceaſed as it became more generally diffuſed through the wards: part of this effect, however, was to be attributed to the inattention of thoſe who carried the pipkins, in putting them too near the faces of the ſick; which cauſed them to inhale the ſtrong vapour, as it immediately iſſued from the cups.

The body-clothes and bed-clothes of the ſick were, as much as poſſible, expoſed to the nitrous vapour during the

* That the fumes of the mineral acids poſſeſſed the property of ſtopping contagion was proved by Guyton as far back as the year 1773, who, by means of the fumes of muriatic acid extricated from the muriat of ſoda (ſea ſalt) by the ſulphuric acid, purified the air of the cathedral of Dijon, which had been ſo much infected by exhumations that they were obliged to abandon the building. The proceſs was afterwards publiſhed under the form of "Inſtructions for purifying the air in the military hoſpitals of the French republic;" a copy of which appeared in the Journal de Phyſique, Ventoſe, an 2. ere Franç. The proceſs conſiſted in removing the patients, heating ſome common ſalt, previouſly moiſtened with water, upon a ſtove, and then pouring ſulphuric acid upon the hot ſalt. In an inſtant the ſulphuric acid begins to act upon the ſalt, combines with its ſoda, and diſengages its acid, which riſes in the ſtate of vapour. The operator then leaves the room, and ſhuts the door; and, after twelve hours, returns, and opens the windows, to admit freſh air.

Dr. Smith deſerves great praiſe for his meritorious perſeverance till he got the uſe of acid fumes introduced into the Engliſh hoſpital ſhips; and his ſubſtituting nitre for common ſalt was a happy improvement; for, though acid fumes were known to prevent infection, there was no proof of their having contributed, at the ſame time, to the recovery of the ſick, till theſe experiments were made according to inſtructions drawn up by him. EDIT.

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fumigation; and all the foul linen removed from them was immediately immerſed in a tub of cold water, afterwards carried on deck, rinſed out, and hung up till nearly dry, and then fumigated before it was taken to the waſh-houſe: a precaution extremely neceſſary in every caſe of infectious diſorder. Due attention was alſo paid to cleanlineſs and ventilation.

It took about three hours to fumigate the ſhip. In about an hour after, the vapour having entirely ſubſided, the ports and ſcuttles were thrown open for the admiſſion of freſh air. It could be plainly perceived, that the air of the hoſpital was greatly ſweetened even by this firſt fumigation. The proceſs was repeated again next morning; and the people employed, being now better acquainted with it, were more expert, and finiſhed the whole in about an hour's time. In an hour afterwards, the vapour having entirely ſubſided, the freſh air was freely admitted into the hoſpital as before. Fewer pipkins were employed for the evening fumigations than for thoſe of the mornings, as the freſh air could not be admitted ſo freely after the former as the latter.

The pleaſing and immediate effect of the fumigation in deſtroying the offenſive and diſagreeable ſmell, ariſing from ſo many ſick crowded together, was now very perceptible, even to the nurſes and attendants; the conſequence of which was, that they began to place ſome degree of confidence in its efficacy, and approached the cradles of the infected with leſs dread of being attacked with the diſorder: ſo that the ſick were better attended, and the duty of the hoſpital was more regularly and more cheerfully performed. In ſhort, a pleaſing gleam of hope ſeemed now to caſt its cheering influence over that general deſpondency, which was before evidently pictured in every countenance, from the dread and horror each individual naturally entertained of being, perhaps, the next victim to the malignant powers of a virulent contagion.

It is a remarkable fact, that from the 26th of November

1795, when the fumigation was firſt reſorted to, till the 25th of December, not a perſon on board was attacked with the fever, though, in the three months preceding, more than one third of all the people in the ſhip had been ſeized with the diſtemper, and of theſe more than one in four were carried off by it; and the probability is, that the ſickneſs and mortality would have gone on, increaſing in proportion to the diffuſion of the contagion, and to the increaſing deſpondency of the people, who conſidered themſelves as ſo many devoted victims.

The advantage of the fumigation was not felt by the ſhip's company and attendants alone, whom it preſerved from the baneful effects of the fever: the ſick and convaleſcents derived almoſt an equal benefit from it. The ſymptoms of the diſeaſe were meliorated, and loſt much of their malignant appearance; and the advantage of a pure air, and free from ſtench, to convaleſcents, may be readily conceived.

Great confidence is always dangerous. It proved ſo on the preſent occaſion. On the 17th of December they imagined themſelves ſo ſecure, that they diſcontinued the cuſtom of fumigating morning and evening, thinking that once a day was ſufficient; on the 25th, one of the nurſes ſuffered a ſlight attack; and on the 26th, a marine, who, for a week before, had been in a ſtate of intoxication, was ſeized with the fever, of which he died. Theſe two accidents gave immediate alarm: they returned again to the practice of fumigating twice a day; and from that time to the extermination of the diſorder, there was not an inſtance of a perſon ſuffering from contagion on board the ſhip.

The ſucceſs of the experiment was not confined to the Union: the power of the nitrous vapour to deſtroy contagion was equally diſplayed on board the Ruſſian ſhips in which it was employed. The ſafety too with which it may be employed, in any ſituation, without inconvenience or riſk of fire, is another great recommendation in its favour.

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From the deſcription that has been given of the proceſs, no perſon can be at any loſs in reſorting to the ſame kind of fumigation. It is only neceſſary to obſerve, for the ſake of thoſe who may not be verſant in chemical purſuits, that the ingredients ought to be pure, and that metal veſſels or rods muſt not be employed. Any kind of metal getting among the ingredients would cauſe the vapour to be very noxious, inſtead of ſalutary. The fumes that riſe ſhould be white; if they are of a red colour, there is reaſon to ſuſpect the purity of the ingredients.

The importance of this diſcovery need not be inſiſted on: it is equally applicable to every ſpecies of putrid contagion, even to the plague itſelf. It ſhould therefore be uſed in all hoſpitals and pariſh workhouſes; and ſhould be conſtantly reſorted to by the proprietors of all large works, on the firſt appearance of infectious diſeaſe among the people employed in them: – indeed, it ſhould be employed even as a preventive in all ſituations, where a number of people, from the nature of their buſineſs, are obliged to be crowded together, or where, from local circumſtances, there are reaſons for ſuſpecting that the purity of the air is injured by noxious exhalations or other cauſes. If there be any circumſtances in which its utility might be called in queſtion, it can only be in caſes of inflammatory diſeaſes; for, in ſuch, ſuperoxygenation has been found hurtful.

XIV. *Deſcription of an improved German Key for extracting Teeth. By Mr. Charles Brown, Surgeon. Communicated by the Inventor.*

THE obſervations with which you accompanied the announcement of my improvement in your laſt number, renders any introduction to the deſcription of the inſtrument unneceſſary.

The handle A (Plate II.) is made of ivory, very large and heavy, with the view of giving greater purchaſe to the operator.

B is a circular milled projection on one end of the lever B K, and serves as a thumb-piece to be pressed into the shank against a spring placed below the same end of the lever; by which means the end K is raised so as to unlock the claw when it is wished to be turned to another side of the instrument.

H shews a section of this part of the instrument, where I represents the spring that is acted upon by pressing down the thumb-piece B, so as to raise the end K of the lever, to discharge it from the notch in the heel of the claw when the latter is wished to be moved to a new position.

C, the claw, is made to come off flat at its lower extremity, and roughed on the inside, which, with the shoulder E, forms a bed to receive the tooth.

D, the bolster, is made concave, to answer to the convexity of the teeth.

G, the heel of the claw, has four, six, or a greater number of notches round it, fitted to receive the end K of the lever.

The improvements introduced into this instrument are such as I hope will be found to remedy the defects complained of in all former ones; and the facility with which a tooth can be drawn by its means, will, I am sure, render its adoption in practice universal, as soon as it shall be generally known.

XV. *On the Use of the new Metal called Chrome, the Oxyd of Chrome, and the Chromic Acid. By Cit. VAUQUELIN. From the* Journal des Mines, *No. XXXIV.*

THE fragility of chrome, the resistance it offers to the action of fire, and the smallness of the masses in which it has hitherto been naturally found, do not leave us any hopes that this metal can ever be of great utility in the arts. This assertion, however, may be going rather a little too far; for a new substance, the properties of which do not at first seem

likely to be of much benefit to ſociety, is ſometimes found, after a certain period, to be applicable to many important purpoſes in the arts and the ſciences.

The acid and the oxyd of this metal, however, may be of the greateſt utility. The former, on account of the beautiful emerald green colour which it communicates, even to enamel, without undergoing any alteration in its ſhade, will furniſh painters in enamel with the means of enriching their pictures, and of improving their art; and the ſecond, by the beautiful cinnabar red colour which it aſſumes and preſerves in its combination with mercury, the orange red colour which it gives with lead, and the carmelite red which it communicates to ſilver, may become exceedingly valuable to painting in oil and in water colours.

It will be no leſs uſeful in chemiſtry, by furniſhing an excellent re-agent to detect the ſmalleſt quantities of mercury, ſilver, and lead, diſſolved in acids, by the different colours it produces in them when its action is aſſiſted by an alkali. But in the ſame manner as this acid ſerves to diſcover the preſence of the above-mentioned metals; the latter, in their turn, may ſerve to diſcover the chromic acid, if it has been previouſly put in a condition to produce the effects before mentioned.

If abundance of the chromic acid ſhould one day be found in any other combination than that of lead; were it extracted by means of the carbonat of potaſh, we might make the red lead artificially, and furniſh, in great plenty, an excellent orange red colour for painting, which is ſold very dear in Siberia, where it is employed for that purpoſe with great effect. In this reſpect great benefit might be derived from the red lead, which is diſſeminated in ſmall cryſtals or laminæ, in the fiſſures of quartzy and free-ſtone matrices*, where they are commonly found, by reducing them

* As many of our readers may not be acquainted with the hiſtory of this red lead, we ſhall give the following ſhort account of it: The foſſil known under

them to powder, and afterwards boiling it in a ſolution of the carbonat of potaſh, and mixing with the nitrat of lead this ſolution, after its exceſs in alkali has been ſaturated with the nitric acid. By this proceſs red lead may be formed as beautiful as the natural, and perfectly free from the matrix.

There is reaſon to preſume that chrome, in the ſtate of an oxyd, or in that of an acid, will be found either free or in combination with other ſubſtances; for the analyſis of the emerald of Peru has ſhewn to me that its colouring principle is ſupplied by the oxyd of this metal; which is a very agreeable preſage for the goodneſs and fixity of this colour, ſince we know that the emerald may be expoſed to the moſt violent degree of heat without being diſcoloured.

I have found alſo that the yellowiſh green velvety cryſtals, which often accompany the red lead of Siberia, are formed of chrome and lead, both united in the oxyd ſtate. In certain fragments of red lead there may be ſeen alſo green cryſtals, which have the ſame form, the ſame dimenſions, and

under the name of *red lead* was diſcovered by M. Pallas, in 1770, in the gold mine of Bereſof, near Ekaterimbourg in Siberia, under the form of priſms with four planes, with, or without, terminating pyramids of a beautiful orange red colour, generally fixed in a quartzy matrix, to which they adhere ſo ſtrongly that it is difficult to detach them.

From the above mine have been procured all the ſpecimens of this ſubſtance preſerved in the cabinets of Europe, which ſhews that it was formerly very abundant; but we are aſſured that for ſome years it has become exceedingly ſcarce, and that it is ſold at preſent for its weight in gold, eſpecially when pure and of a regular form. Specimens which do not poſſeſs a regular form, or which have been reduced to fragments, are applied to the purpoſes of painting, in which art it is exceedingly valuable, on account of its beautiful orange yellow colour, its durability in the air, and the facility with which it can be mixed with oil. Profeſſor Pallas expreſſes himſelf thus, in ſpeaking of this mineral, in his Travels during the year 1770, under the article of the gold mine of Piſchminſkoi, vol. ii. p. 235. "A very remarkable kind of mineral red lead, not found in any other mine of the Empire or elſewhere, is dug up here. This lead ore is heavy, of different colours, ſometimes that of cinnabar, and very tranſparent.

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and the ſame diſpoſition in the matrix, as thoſe of the red lead, and which are only a combination of the oxyd of chrome and the oxyd of lead. It is probable that theſe combinations originally exiſted in the ſtate of the chromate of lead, and that at length a portion of oxygen taken away by cauſes with which we are unacquainted, made them paſs to that of oxyd, and converted the red into green.

XVI. *Experiments and Obſervations on the Phoſphoreſcence of the Luciole*, Lampyris Italica. *By Dr.* CARRADORI. *From the* Annales de Chimie.

THESE winged inſects, which during the calm nights of ſpring fly about in the air, where they appear like ſparks of fire, that attract the curioſity of children and afford them amuſement, cannot fail in a more particular manner to excite the curioſity of the philoſopher.

It adheres in long or ſhort cryſtals in the fiſſures of quartz, as well as on the edges of the ore, which is a ſandy kind of ſtone. It has very frequently, and wherever the ſpace has permitted, the ſame ſize and the ſame priſmatic form, with four flat facets, having two extremities irregularly blunted. It is found alſo in ſmall irregular twiſted pyramids, attached to a quartz like ſmall rubies. When well ground with water, it gives a beautiful ſubſtance of a dark yellow colour, which may be employed in miniature painting. In all the experiments made on this lead ore in the laboratories of Ekaterimbourg, a ſmall grain of ſilver has always been obtained. It is difficult at preſent to procure the quantity neceſſary for extenſive experiments, as the workmen cannot labour where this lead ore is found, for want of good air. There are found in the mixed matrices of quartz, where this rare and curious mineral is formed, ſmall cryſtals pointed at both ends, and of the colour of ſulphur. They reſemble native ſulphur, and they are conſidered as ſuch by the miners; but they do not burn in the fire, and do not flaſh when applied to a flame, like the lead ore. They may conſiſt perhaps of metallic ſpar, but it is difficult to procure the quantity neceſſary for experiments. Theſe ſmall cryſtalliſations are found both on the quartz and the ſand-ſtone." EDIT.

The light of the luciole does not depend on the influence of any external cauſe, but merely on the will of theſe inſects. While they fly about at freedom their ſhining is very regular; but when they are once in our power, they ſhine very irregularly, or do not ſhine at all. When they are moleſted they emit a frequent light, which appears to be a mark of their reſentment. When placed on their backs they ſhine almoſt without interruption, making continual efforts to turn themſelves from that poſition. In the day-time it is neceſſary to torment them in order to make them ſhine, and thence it follows that the day to them is the ſeaſon of repoſe. The luciole emit light at pleaſure from every point of their bellies, which proves that they can move all the parts of their viſcera independently of each other. They can alſo render their phoſphoreſcence more or leſs vivid, and continue it as long as they pleaſe.

The faculty of ſparkling does not ceaſe on their bellies being torn, or opened by an inciſion. Carradori ſaw part of the belly of one ſeparated from the reſt of the body, the light of which was almoſt extinguiſhed, grow luminous all of a ſudden for ſome ſeconds, and afterwards become gradually extinct. He ſometimes ſaw a like portion, which had been ſeparated, paſs ſuddenly from the moſt brilliant ſtate to that of total darkneſs, and afterwards reſume its former brightneſs. Carradori aſcribes this phenomenon to a remainder of irritability, or a ſtimulus produced by the air, which appears the more probable, as a mechanical irritation produced the ſame effect.

A ſlight compreſſion deprives the luciole of their power of ceaſing to ſhine. The author is inclined to believe that the movement by which they conceal their light, is executed by drawing back their phoſphoric ſubſtance into a particular membrane or tunic. He ſuppoſes alſo that the ſparkling conſiſts in a trembling or oſcillation of the phoſphoric maſs. He is of opinion that there is no emanation of a phoſphoric ſubſtance, and that the whole phenomenon takes place in

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the interior part of the luminous viſcera. When the ſhining is at its greateſt degree of height, it is ſo ſtrong that a perſon may by it eaſily diſtinguiſh the hours on the ſmalleſt watch, and the letters of any type whatever.

The phoſphoric part of the luciole does not extend farther than to the extreme rings of the belly. It is there encloſed in a covering compoſed of two portions of membranes, one of which forms the upper, and the other the lower part of the belly, and which are joined together. Behind this receptacle is placed the phoſphorus, which reſembles a paſte, having the ſmell of garlic, and very little taſte. The phoſphoric matter iſſues from a ſort of bag on the ſlighteſt preſſure; when ſqueezed out, this matter loſes its ſplendour in a few hours, and is converted into a white dry ſubſtance. A portion of the phoſphoric belly put into oil, ſhone only with a feeble light, and was ſoon extinguiſhed. In water, a like portion ſhone with the ſame vivacity as in the air, and for a much longer time. The author thence concludes that the phoſphoreſcence of the luciole is not the effect of ſlow inflammation, nor of the fixation of azotic gaz as Gœttling thinks, as the oil in which they ſhine does not contain a ſingle air-bubble: beſides, the phoſphorus of theſe inſects ſhines in a barometrical vacuum. The obſervation made by Foſter, that the luciole diffuſed a more vivid light in oxygen gaz than in atmoſpheric air, does not, according to Carradori, depend upon a combuſtion more animated by the inſpiration of this gaz, but on the animals feeling themſelves, while in that gaz, in a better condition. "Whence then ariſes," ſays the author, "the phoſphoric light of the luciole? I am of opinion," adds he, "that the light is peculiar and innate in theſe inſects, as ſeveral other productions are peculiar to other animals. As ſome animals have the faculty of accumulating the electric fluid, and of keeping it condenſed in particular organs, to diffuſe it afterwards at pleaſure, there may be other animals endowed with the faculty of keeping

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in a condenſed ſtate the fluid which conſtitutes light. It is poſſible that by a peculiar organiſation they may have the power of extracting the light which enters into the compoſition of their food, and of tranſmitting it to the reſervoir deſtined for that purpoſe, which they have in their abdomen. It is not even impoſſible that they may have the power to extract from the atmoſpheric air the luminous fluid; as other animals have the power of extracting from the ſame air, by a chemical proceſs, the fluid of heat."

Carradori diſcovered that the phoſphoreſcence of the luciole is a property independent of the life of theſe animals, and that it is chiefly owing to the ſoft ſtate of the phoſphoric ſubſtance. Its light is ſuſpended by drying, and it is again revived by ſoftening it in water; but only after a certain time of deſiccation. Reaumur, Beccaria, and Spallanzani obſerved the ſame thing in regard to the *pholades* and the *meduſa*.

By plunging the luciole alternately into lukewarm and cold water, they ſhine with vivacity in the former, but their light becomes extinct in the latter; which, according to the author, depends on the alternate agreeable and diſagreeable ſenſation which they experience. In warm water their light diſappears gradually. Dr. Carradori tried on the luciole and their phoſphorus the action of different ſaline and ſpiritous liquors, in which they exhibited the ſame appearances as other phoſphoric animals. Theſe laſt experiments prove that the phoſphoric matter of the luciole is only ſoluble in water.

XVII. *On a new Insect called* Antiodontalgicus, *and the Property possessed by it, in common with some other Insects, of curing the Tooth-ache.*

PROFESSOR GERBI, in a small work published at Florence, in 1794*, gives the description and figure of an insect, which is a kind of *curculio*, that, from its property of allaying the tooth-ache, has received the epithet of *antiodontalgicus*, and which is found on a species of thistle, *carduus spinosissimus*, of which a figure is likewise given. This thistle is compared by Professor Gerbi to the thistle of Boujarti, and is perhaps a variety of the *cnicus*. Its flowers, when analysed, gave the acid of galls, the muriatic acid, oxalat of lime, extractive matter, and a very little resin. On the bottom of the calyx, which supports the flowers, there are often found excrescences like the gall-nut, which are at first spheroidal, afterwards cylindric, and at length assume the figure of two hemispheres: they consist of the like component parts, but contain more resin, and far more oxalat of lime; as the gall apple of the oak, according to the experiments of M. Branchi, which are here mentioned, contains more of the acid of galls than the bark and other parts of the oak, in which he could discover no sulphuric acid. The insect, according to the author's observations, eats not only the parenchyma, but also the vessels and fibres of the leaves. The egg, before the worm makes its appearance, is nourished by the sap of the plant, and of the above excrescences, in which it resides, by means of the attractive power that the egg possesses for certain vegetable juices and substances. The excrescences arise by the accumulation of a solid substance, which is precipitated from the nourishing juices of the thistle, diminished by nourishing the egg and the worm. This insect, the eggs of which are

* Storia naturale di un nuovo insetto di Ranieri Gerbi. Florence 1794. 8.

deposited in these excrescences, is, together with the *curculio* of the centaury, a new species. It is of a longish figure; covered below with short yellow hair, and above with golden yellow velvety spots. Its corslet is variegated with specks; and the covering of its wings with specks and stripes. It has a short proboscis, and shews some likeness to the *curculio villosus* of Geoffroy. Its larva represents a sort of ichneumon. By chemical analysis it exhibits some traces of common salt; by distillation with a strong dry heat, some volatile lixivious salts; and it contains, besides these, some gelatinous, and a little sebaceous and slimy extractive matter. If about a dozen or fifteen of these insects, when in the state of larva, or even when come to perfection, be bruised and rubbed slowly between the fore-finger and thumb until they have lost their moisture, and if the painful tooth, where it is hollow, be touched with that finger, the pain ceases sometimes instantaneously. This power or property the finger will retain for a year, even though it be often washed and used. A piece of shamoy leather will serve equally well with the finger. Of 629 experiments, 401 were attended with complete success. In two of these cases the hollow teeth arose from some fault in the juices: in the rest they were merely local.

If the gums are inflamed, the remedy is of no avail. According to Professor Gerbi, the hollowness in the teeth arises from an acrid matter which irritates the nerves, and which is neutralised by the juices of the insect.

This property of curing the tooth-ache has been discovered in other insects, as appears by a paper of Dr. Corradori, inserted in the *Giornale fisico-medico* of Brugnatelli, Part I. The experiments were made in the town of Prato, where the property of some insects to cure the tooth-ache had been known among the peasants, even before any thing was published on the subject by Gerbi and Comparini. A certain Luigi Mari is here made to assert, that he laid hold of about a dozen of these insects with his fingers, and heaped them

them over each other, by which means his fingers, for a whole year, though he daily washed his hands, were endowed with the property of curing the tooth-ache, by merely touching the affected tooth. The author of the letter affirms, that the following method will always afford relief. The insect described by Rossi, in his *Fauna Etrusca*, under the name of *carabus chrysocephalus*, is to be held for some minutes between the thumb and the fore-finger, and the diseased tooth and the gums are to be afterwards touched with that finger. If the pain does not cease on the first touching, the finger is to be well dried, and must be applied to the insect again, and then to the tooth; and this is to be continued alternately till the pain ceases entirely. M. Cypriani asserts, that he cured every case of the tooth-ache, without exception, by this method, within the space of eight or nine minutes.

More insects possess the like property, and experiments made with the following were equally successful. The *carabus ferrugineus* of Fabricius; the *coccinella septem punctata* (the lady bird); the *chrysomela populi*, and the *chrysomela sanguinolenta*. It would appear that this property belongs to various kinds of the *coleoptera*.

The idea of these insects being endowed with the property of curing the tooth-ache is not confined to Italy; for Dr. Hirsch, dentist to the court of Weimar, asserts (*Verkundiger*, September 24, 1798,) that he employed them with the happiest effect, except in some cases where his patients were females. Dr. Hirsch says, that he took that small insect, found commonly among corn, *coccinella septempunctata*, and bruised it between his fingers. He then rubbed the fingers with which he had bruised it, till they became warm at the points, and touched with them the unsound parts of the gums, as well as the diseased tooth. Dr. Hirsch adds, that he made the same experiment a few days after, with equal success, though he had not bruised a new insect with his fingers. He seems to think, though the

other authors we have quoted do not mention the circumstance, that, to insure the efficacy of the process, the insect should be alive; because when dead, its internal parts, in which he presumes the virtue chiefly resides, become dried up, leaving only the wings and an empty shell; and therefore proposes to physicians, to turn their attention to the finding out of some method for preserving the virtue of the insect, so that its efficacy may be in full vigour throughout the year.

Professor Gerbi's work on the virtues said to be possessed by the *antiodontalgicus* was first announced in this country by Dr. Beddoes, in a paper published in the *Monthly Magazine for November* 1796; but in such a cautious manner as evidently to shew that he was afraid an improper degree of credulity might be ascribed to him. He has however proposed, that impregnated shamoy leather should be imported by the way of Leghorn; but as the virtue seems to belong to several coleoptera in this country, such an importation is unnecessary. The impregnating of leather (other substances might also be tried) by rubbing it with the insects, seems a good way of realising what has been proposed by Dr. Hirsch.

The fear of having credulity imputed should never hinder the publication of physical facts, announced by men of learning and reputation, however unaccountable they may appear; for, no one will pretend that we already are acquainted with all the laws of nature: on the contrary, they ought to be made generally known, that their truth or falsehood may be established by numerous experiments, especially when, as in the present instance, the means are so much within the reach of every person who may wish to determine the matter for himself.

While on the subject of tooth-ache, we may add, for the information of those who have not perused Dr. Beddoes' ingenious publications on the medicinal effects of factitious airs, that charcoal has been found to relieve the pain. Mr. Sandford, to a letter on the virtues of charcoal, addressed to

Dr.

Dr. Beddoes, ſubjoins the following P. S.—" A lady to whom I recommended the uſe of charcoal-powder as a dentifrice, was ſubject at times to violent tooth-ache, from one of the dentes ſapientiæ that was become carious. She one day, when in great pain, filled the cavity of the tooth with the charcoal dentifrice, and was ſurpriſed to find herſelf in a few ſeconds free from pain. It might probably be urged, that any ſubſtance filling the hollow of the tooth mechanically, and thereby preventing the acceſs of atmoſpheric air, would produce the ſame effect: but I am rather inclined to attribute the *temporary* good effect experienced in this caſe, to the power of the charcoal *.

XVIII. *Deſcription of the Gazometer invented by M. VAN MARUM and his Apparatus for producing Water by the Combuſtion of Hydrogen in Oxygen Gaz. From the* Annales de Chemie, *Vol. XII.*

THE veſſel 11 inches in diameter, containing the air or gaz to be employed, is repreſented by A. The mouth of it is cloſed by a braſs cover ſcrewed upon it, and furniſhed with three cocks B, C, D. On the cock B is ſcrewed a copper ſyphon E F, having its end F ſcrewed upon another braſs tube open at the bottom, repreſented by the dotted lines G G, and which deſcends within the braſs cylinder H, which is open at the top. To the lower part of the cock B is cemented a glaſs tube I I, which is open at the lower end near the bottom of the glaſs. When the cock B is opened, the tubes G G, F E, I I, make only one ſyphon; from which, when it is completely full, and the water in both veſſels does not ſtand on the ſame horizontal line, the water

* Any of our readers who try this ſimple application, will oblige us by communicating the reſult, to determine, whether charcoal be a remedy for the tooth-ache, or whether we muſt attribute the above caſe to a natural ceſſation of pain.

will be conveyed from that veſſel where it has the greateſt height into the other, until the water in both be of the ſame height. For example, when the water in the cylinder H ſtands at K, and in the glaſs A at L (in which caſe we muſt ſuppoſe the cock D open that the air may eſcape), the water will continue to flow from H into A till it ſtands at the ſame horizontal line.

The higher the water K is raiſed in H the greater is the weight of the column, and the quicker the eſcape of the air. By theſe means M. Van Marum obtains what Meſſieurs Lavoiſier and Meuſnier call preſſure in the gazometer, merely by the higher level of the ſurface K over L. That the height of the water in H may be obſerved, a glaſs tube M M about $1\frac{1}{2}$ inch in diameter, and connected with it, is placed between H and A, in which the water will always have the ſame altitude as in H. An ivory ſcale, divided into inches and lines, faſtened to a cork ball, floats upon this water, and ſerves to ſhew the difference of the altitudes of the water at L and K.

The cock N, fitted to the top of the open cylinder O, ſerves, together with the cock P, to preſerve the preſſure perfectly uniform. Suppoſe, for example, that a conſtant preſſure of water of two inches high was required, as much water muſt be ſuffered to flow continually into H as is equal in bulk to the quantity of water that this preſſure of two inches forces from H into A. For this purpoſe, fill the cylinder O with water to the height of four inches, and turn the cock N until the preſſure of the four inches of water, which is maintained by the cock P, forces through N into H a quantity of water exactly equal to the quantity that the preſſure of two inches forces from H into A. The index Q of the cock N, and the ſcale R S, ſerve to give to the cock N the exact opening required; this having been determined beforehand, and the ſcale divided accordingly.

The cock N begins to open when the index R is turned towards S; and, for this reaſon, the gradation of the ſcale

begins

begins at R. When the index is in a vertical position, the cock is completely open.

As it might perhaps be difficult to give to the cock P, which is connected with a ciſtern of water, ſuch an opening as not to permit more water to flow through it than neceſſary for preſerving a height of four inches in the cylinder O, a waſte pipe T T is applied at this height, which ſuffers all the ſuperfluous water of the cock P to run off. In order that it may be more conveniently obſerved, whether the cock P has a ſufficient opening for maintaining a four-inch column of water, a glaſs tube U, marked at the required height, is applied on the outſide, connected with the cylinder O.

To fill this gazometer, nothing more is neceſſary than to let water run out from the cylinder H. For this purpoſe, the cock V muſt be opened until the water in H ſtands lower than that in A. The cock D, and the cock W, ſtanding over the bell X, are then opened. The air contained in X riſes through the pliable tube Y Y into A. This filling may be continued, by leaving the cock V open, that the water which proceeds through the ſyphon from A to H may run out, and the ſurface of the water in H ſtand always a few inches lower than that in A. At the ſame time the bell X is to be ſupplied with new gas in the uſual manner. When the operation of filling is completed, the cock V is to be ſhut, and that at Z opened; the lower part of the opening of the key of which is in the ſame horizontal line with zero of the ſcale, which ſhews in cubic inches the altitude of the water in A. By theſe means the water falls no lower in A than to the above-mentioned line; and A is conſequently filled with air to the beginning of the ſcale *a b*. Before the cock D is ſhut, care muſt be taken that the water in the bell X do not ſtand higher than the water that ſurrounds it in its tub, which may be effected, if the bell be depreſſed in the tub till the internal and external altitude of the water be perfectly equal; and the air in A will then be of the ſame denſity as that of the atmoſphere.

The gazometer is furniſhed with a thermometer *f g*, which is ſo cemented with ſealing wax into a piece of copper *e*, that the bulb of it is within the gazometer. By theſe means the temperature of the air uſed may be known, and its weight accurately aſcertained. The tripod on which the gazometer ſtands facilitates the adjuſtment of the apparatus, and a braſs bottom ſcrewed upon the tripod is furniſhed with a braſs rim or lip to receive the glaſs A and keep it faſt in its place. Exactly at the upper level of this rim the ſcale *a b* commences. The ſcale is of ivory, and faſtened on a ſlip of braſs, and at the ends are two ſquare pieces of braſs, by which it is made faſt, by means of two ſcrews, to the cover at the top, and to the before-mentioned lip. The ſcale is divided in the common method, by pouring equal meaſures of water (ſuppoſe 2 cubic inches) into the glaſs, and marking the height of each on the ſcale, or, as the glaſſes are nearly cylindrical except at the neck, it may be ſufficient to uſe at once 32 cubic inches, and divide the height into 16 equal parts; the neck of the veſſel muſt be divided, as above, by 2 cubic inches at a time.

To explain the uſe of the gazometer it is only neceſſary to ſhew how the ſyphon G F E I is filled at the commencement of the experiment. This is done almoſt in the ſame manner in which M. Lavoiſier filled the ſyphon, which he employed in order to fill the lamp uſed in the experiment reſpecting the combuſtion of oil *. The method is as follows:—The two cocks N and P are to be opened at the ſame time, and to be kept ſo until the cylinder H is completely filled with water. The pipe G G, which is open both above and below, as the crooked pipe F E is not yet joined to it at F, will be filled at the ſame time with the cylinder. The tube G G is then to be ſhut below by the cock *b*. This cock is fixed into a piece of braſs, which is faſtened in the cylinder H by four ſcrews, the heads of which may be ſeen at *i, i, i, i*. To this piece of metal, made hollow in a perpendicular direc-

* See his Chemiſtry, Paris edit. 1789, page 495.

tion,

tion, is ſoldered the tube G G, which touches the inſide of the cylinder H, and is made faſt at the top by a piece of braſs, which is ſcrewed to the interior edge of the cylinder by two ſcrews, the heads of which are ſeen at K. As ſoon, therefore, as *b* is ſhut, the crooked pipe F E is to be ſcrewed to G G and the cock B. The cock B is ſhut, the ſcrew *j* is to be unſcrewed to open the tube at that place, and water poured by means of a funnel into the orifice thus opened. As ſoon as the pipe E F is full, the orifice is again ſhut. The cock *b* being then opened, and afterwards B, ſo much water runs from H, through the ſyphon G F E I, that A will be completely filled, if care be taken, by opening the cocks N and P, that the water in H ſtand always higher than that in A. To haſten the filling of the glaſs A, it will be convenient to fill the cylinder H almoſt entirely.

In order that the bent pipe E F may be faſtened air tight to the cock B, and the tube G G, without turning it round, each end of E F is ground to fit conically the places intended to receive them, and is preſſed home by the following contrivance: Fig. 2. repreſents a ſection paſſing through the axis of this part of the apparatus. The part *a a*, which is furniſhed with a ſhoulder, is ſlipped into the hollow *c* of the cock B; and the female ſcrew *d d*, by means of its ſhoulder *f f*, when turned upon the ſcrew *e e*, preſſes the conical part *a a* into *c*. The conical piece of the other end F of the pipe E F, is adjuſted in the ſame manner into a piece of copper ſoldered to the tube G G. All the tubes of this apparatus are adjuſted to their reſpective cocks in the ſame manner. It will be ſufficient to greaſe ſlightly the ſurface of any of theſe conical pieces, before it is put in its place, to prevent all communication with the air of the atmoſphere.

The balloon, for the compoſition of water, placed upon its tripod between the gazometers, differs from that of Lavoiſier, principally in the manner of ſhutting it in order to prevent the entrance of air. For this purpoſe there is a rim of copper *a a* (*fig.* 3.) fixed on the neck of the balloon, by means

means of plaſter of Paris, which, that it may afford no paſſage to the atmoſpheric air, is coated over with common cement above the edge of the band *a a*, as may be ſeen at *b b*; and this cement is covered with a ſtripe of linen *c c*, a quarter of an inch in breadth, dipped in the white of an egg mixed with lime. The rim of copper *a a*, has a copper ring *d d*, ſoldered to it at right angles; and on which the plate of copper *e e* is ground, in ſuch a manner that it is ſufficient to greaſe ſlightly the upper ſurface of the ring *d d*, when a vacuum is to be made, taking care to apply ſo little greaſe that it may not penetrate into the balloon. To exhauſt the balloon there is a cock which communicates by a bent tube with an air pump placed behind the balloon; and to prevent the working of the air pump from ſhaking the balloon, this bent tube is made in part of elaſtic gum.

The plate of copper *e e* is kept down upon the ring *d d* by ſix ſcrews, two of which may be ſeen at *f f*, placed at equal diſtances around the neck of the balloon, and by means of which the plate *e e* may be made to preſs very ſtrongly on the ring *d d*, on turning theſe ſcrews by the help of a key. Both of the gazometers are made to ſhut in the ſame manner.

The inferior ſurface of the plate *e e* is covered, as far as it covers the opening of the balloon, by a thin plate of pure ſilver, in order that the vapours formed during the experiment may not touch the copper. For the ſame reaſon the bent tube *l m*, in the balloon, is alſo made of pure ſilver. At the extremity of this tube is a piece of platina, having a very ſmall aperture, ſcarcely ſufficient to afford a paſſage to a very fine needle. The part *n*, which ſerves as a conductor to kindle the hydrogen gaz by an electric ſpark, is alſo made of platina, as far as it is not enclоſed in the glaſs tube *o o*, by which it is inſulated. I have preferred platina for theſe two parts, in order to prevent the oxydation of the metal, which the heat produced by the combuſtion of the hydrogen gaz in oxygen might occaſion. The tubes *p p p p*, which

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ſerve to conduct the gazes into the balloon, are made of glaſs, and cemented into the copper tubes *q q*, which have conical extremities like thoſe above deſcribed to fix them on the cocks. Theſe glaſs tubes are raiſed a little above the edge of the cylinder H, to prevent the water from paſſing into the balloon, ſhould you happen to fill one of the glaſſes A, and neglect to ſhut well the cock C.

It will be proper to make the glaſſes A, pretty large, if they can be procured ſo, in order that the gazometers may contain the more air. It will however be beſt to make them rather high (28 or 30 inches) than wide, in order that the exactneſs of the ſcale *a b* may not be diminiſhed. M. Van Marum anſwers an objection which he ſays may poſſibly be urged againſt this apparatus, namely, that the combuſtion is obliged to be ſuſpended every time that the gazometers need to be repleniſhed with either oxygen or hydrogen. He ſays this is no ſolid objection, as he finds that, ſince he uſed platina for conveying the electric ſpark, he never fails to kindle the hydrogen the firſt moment it enters the balloon; and that, beſides, the combuſtion of a cubic foot of hydrogen gaz, in order that the water produced may not contain acid, ought to take ſix hours. Thoſe who wiſh to make experiments of longer duration, have only to attach two other gazometers; by which means, while the one ſet is emptying, the other may be repleniſhed.

For this purpoſe it will be neceſſary to have, inſtead of the cock *u*, two cocks Z 1, Z 2, (fig. 4) ſcrewed to a piece of copper fixed to the cover of the balloon; and by which each of theſe cocks has a communication with the bent tube *l m*, by means of two holes *x*, *x*, which proceed obliquely through this piece, and end in the tube *l*. The firſt gazometer, which communicates with the balloon by the cock Z 1, being almoſt emptied, the cock Z 2, of the ſecond gazometer, may be opened, after having made the preſſure in the ſecond gazometer equal to that in the firſt. It is evident that the preſſures of theſe two gazometers being equal,

equal, the velocity with which the hydrogen gaz enters the balloon, will not experience the leaſt change. When the firſt gazometer is entirely empty, the cock Z 1 is ſhut, and the firſt gazometer is filled; and the cock Z 1 is not opened till a little before the ſecond gazometer is totally emptied. The ſecond gazometer is then to be filled again, and in this manner the combuſtion of hydrogen gaz may be continued by theſe gazometers to any length of time without interruption. To introduce the oxygen gaz without interruption into the balloon by means of two gazometers, it will be ſufficient that the two cocks, which form a communication between the gazometers and the balloon, be immediately fixed in the covering of the latter.

Both halves of the apparatus being ſimilar, the letters of reference are only marked on that part which is given in outline upon the plate.

XIX. *On the Choice of Steel, and the Methods of hardening and tempering it. By Mr. S. VARLEY. Communicated by the Author.*

IT ſeldom happens that theory and practice are united in the ſame individual: the man of ſcience often labours under a great diſadvantage (notwithſtanding his excellent theories) for want of the experience of the practitioner, and the practical man ſeldom has a theory to guide him. He knows from experience that certain things or operations will produce a particular effect, but can give no reaſon why. His application to buſineſs has not allowed him time to ſearch into cauſes, nor to reaſon upon effects; and it would be difficult to perſuade this uſeful claſs of men how much theoretical knowledge might be gained by applying their leiſure hours to ſtudy, and how much pleaſure might be derived from thus uniting theory with practice, which ſo mutually aſſiſt each other.

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I was led to theſe reflections by reading Mr. Collier's paper on iron and ſteel (inſerted in the firſt number of your excellent magazine), in which he gives ſome methods of hardening and tempering ſteel; and concludes by ſaying, that the ſprings for penknives are covered over with oil before they are expoſed to the fire to be tempered, but gives no reaſon for that application. I ſhall therefore endeavour to ſupply that defect, and add ſome other obſervations on the management of ſteel, conceiving that any uſeful hint on a ſubject ſo univerſally applicable to the manufactories of this country will be of general benefit. It is a ſubject of the firſt importance to the practical mechanic: take this uſeful ſubſtance away, and he would be utterly incapable of proceeding one ſtep forward, and would ſoon be convinced that ſteel is of more value than gold itſelf.

For ordinary purpoſes, the method noticed by Mr. Collier will anſwer very well, both for hardening and tempering; but in many caſes it is neceſſary that the ſteel ſhould be of the beſt quality, and be both hardened and tempered in ſuch a manner as to preſerve the greateſt hardneſs poſſible without brittleneſs; and ſteel is of more or leſs value in proportion as it poſſeſſes this property in a greater or leſs degree.

Steel, when ſoft, can be wrought into almoſt any form as well as iron, welding excepted, of which the better ſorts, particularly caſt ſteel, are incapable. It can be forged, filed, turned in a lathe, drawn into wire, rolled into large plates, &c. &c. and, when by theſe means brought into the deſired form, it can then be made ſo hard as to be capable of cutting the hardeſt ſubſtances (the precious ſtones excepted), while at the ſame time it is almoſt proof againſt being itſelf worn by friction: but in this ſtate it is brittle, like all other hard ſubſtances, and for many purpoſes muſt have this brittleneſs leſſened, and this is what is termed by workmen, tempering, and conſiſts in giving it certain degrees of heat according to the temper deſired, which may be produced in any degree until the whole effect of hardening is deſtroyed, and the ſteel

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is reduced to its ſoft ſtate. On each of theſe operations I ſhall offer a few remarks, proved by long experience; and, firſt, on the choice of ſteel for ſuch purpoſes as require the beſt that can be procured for making cutting inſtruments, ſuch as gravers, punches, turning tools, chiſels, &c. &c. to be employed in turning or cutting tempered ſteel, and ſubſtances that are too hard to be cut by tools made of ordinary ſteel: for theſe purpoſes caſt ſteel is undoubtedly the beſt; but even this ſort differs in quality.

The general mode of chooſing ſuch as is moſt ſuitable for the above purpoſes is to break a bar, and obſerve its fracture, and to ſelect the cloſeſt grained; but this mode is not always certain, owing to the difference made in the fracture by the ſteel being hammered under a greater or leſs degree of heat, ſteel being much improved by being hammered under a low heat, and even when cold; and when overheated, being quite ſpoiled for the above purpoſe. It is owing to this circumſtance that the beſt ſorts of caſt ſteel are incapable of being welded as above mentioned. Another method is, to harden with as low a heat as poſſible a piece of ſteel, and then to break it, and obſerve its fracture: but this is not wholly to be depended upon; for ſome ſteel breaks with a very cloſe grain, and yet is not of a good quality. But the ſureſt method is to have one end of a bar drawn out into a ſmall rod under a low heat, an obſcure red for inſtance, or but little above; then heat it as before, and ſuddenly plunge it into pure cold water: if it proves hard, and requires a great force to break it, it is good, let its fracture be what it may*; and I have always found that the ſpecimens that hardened with the loweſt heat, and when in that ſtate required the greateſt force to break them, proved the beſt ſteel. Having thus ſelected ſteel fit for the required uſe,

* This circumſtance deſerves particular attention, as workmen, in general, reject ſuch ſteel as breaks with a coarſe fracture, even though it be of ſuch a quality as to require a great force to break it, after being hardened.

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and, with the precautions already noticed, given it the proper form, it may be hardened; but the ſame method will not anſwer for all purpoſes. Some pieces, from their ſize and figure, are very difficult to be hardened; if they are large, they heat the water in immediate contact with them, and the heat is communicated to the reſt of the water, ſo faſt that it prevents the pieces from being cooled quick enough to produce the deſired effect: this is in part prevented by continually moving the piece about in the water; but when too large to be hardened by this method, a ſtream of water muſt be employed; and for ſuch pieces as the face of large anvils, a birch broom is uſed with advantage to break the bubbles that are formed by the continual diſengagement of air, and which, if not ſwept away, would prevent that intimate contact and uniform ſucceſſion of the ſtream neceſſary to produce the degree of hardneſs required. Other articles, from their length, are difficult, and almoſt impoſſible to be made hard without bending, or otherwiſe altering their figure: this circumſtance occaſions a great deal of trouble; and many a piece of work is ſpoiled, after a good deal of labour has been beſtowed upon it. The method that has ſucceeded beſt with me is, either to incloſe the piece or pieces intended to be hardened in an iron caſe or box, open at one end (for the more ready dropping the pieces into the water), and giving it a ſlow, yet regular heat; then to take the caſe out of the fire, and drop the pieces into the water in ſuch manner as will allow them to come as little as poſſible in contact with the air. This method anſwers two good purpoſes at once, cauſing the heat to be more equally applied, and preventing the contact of the air, and of courſe any ſcaling; and when the work has been poliſhed and well defended from the air, it comes out nearly as clean as it was before. When the greateſt poſſible hardneſs is required, it may be obtained by uſing quickſilver inſtead of water; but this can only be employed for ſmall articles. For ſome purpoſes ſteel is required to have a ſuperior degree

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of hardneſs given to its ſurface, ſuch as in the caſe of files, &c. This is obtained by uſing a coarſe powder made of leather ſlightly burned, hair or horn, either in raſpings or in powder: this is mixed with a little common ſalt, and the files, when juſt red hot, are thruſt into a heap of this powder, ſome of which adhering to their ſurface is carried into the fire with them, and gives them a caſe hardening: the ſalt fluxes upon their ſurface, and defends them from the air while paſſing from the fire into the trough of water, into which they are plunged to harden. The workmen ſay, the longer this water is uſed for this purpoſe the better.

We are now come to the laſt proceſs called tempering, for one method of which ſee Mr. Collier's paper; but that method cannot be conveniently applied in all caſes, and has ſeveral diſadvantages, ſome of which I ſhall mention. Firſt, each piece muſt be made bright that the change of colour may be better ſeen, and muſt be heated ſingly or nearly ſo; and pieces of irregular figure cannot be made to receive an equal degree of heat in all their parts, ſo that ſome will be ſofter than others. Theſe circumſtances would retard the manufactory of many articles very much, and prevent their being afforded at the preſent prices, ſuch as the ſprings of gun locks, door locks, various articles in clock and watch work, &c. &c. The neceſſity of making them bright enough to mark the change of colour is obviated by ſmearing them with oil or tallow, which helps to apply the heat more uniformly, and marks the temper as well as by obſerving the colour, or nearly ſo; or by putting the things to be tempered into a proper veſſel, and adding ſo much oil or tallow as will cover them, and then holding them over the fire or the flame of a lamp until a ſufficient heat is given. By this means the moſt irregular pieces may be uniformly heated, and great numbers may be done at one time, and with great certainty: thus are clock and watch pinions, watch verges, balances, &c. tempered; ſometimes many dozens at once; and no more time is neceſſary for the whole

whole than would be for one ſingle article. The requiſite temper may be known by the following circumſtances: When ſuch a heat is given that the tallow is firſt obſerved to ſmoke, it indicates the ſame temper as that called a ſtraw colour: this will reduce the hardneſs but little; but if the heat is continued until the ſmoke becomes more abundant, and of a darker colour, it will be equal to a brown, and indicates a temper that may be wrought—that is, which may be turned or filed, but with difficulty, and only when a mild ſort of ſteel is employed. If the tallow be heated ſo as to yield a black ſmoke, and ſtill more abundant, this will denote a purple temper; and if the ſteel is good, it will now work more pleaſantly, though ſtill hard enough to wear well in machinery. The next degree may be known by the tallow taking fire if a lighted body is preſented to it, but yet not ſo hot as to continue to burn when the light is withdrawn; this would equal a full blue colour. Increaſe the heat till the tallow continue to burn, being once lighted, and this will denote a pale blue: and if the whole of the tallow be allowed to burn away, or to burn dry, as the workmen call it, it gives what clock-makers moſtly uſe for their work. Farther tallow is uſeleſs; a ſmall degree of heat more would juſt be ſeen in a dark place, or the loweſt degree of a red heat: ſuch is the temper given to the ſprings for coaches, &c. Thus I have given a reaſon why oil or tallow is made uſe of, and given you the parallel degrees of temper which by a dry heat are obſerved by the change of colour only. The method of hardening in quickſilver is of great uſe where a ſuperior degree of hardneſs is required; and good ſteel ſo hardened, when the precautions before mentioned are duly attended to, will cut glaſs like a diamond, and turn or cut other ſteel at ſo high a temper as to differ but little from quite hard.—Perhaps at a future time I may give you a method by which this hardeſt of ſteel may alſo be worked with conſiderable eaſe, and the caſes in which I have applied it to advantage.

INTELLIGENCE.

LEARNED SOCIETIES.

GERMANY.

ON the 9th of Auguſt laſt the Academy of Sciences at Berlin held a public ſitting in honour of his Majeſty's birth-day, on which occaſion the following prize-queſtions were announced:—

MATHEMATICS.

The mathematical claſs had propoſed for the year 1798 the following prize-queſtion:

"As the labours of the ableſt aſtronomers have left ſeveral points reſpecting the variation in the obliquity of the ecliptic to be explained, the Academy invites the Learned to turn their attention to this ſubject once more, and will adjudge the prize to that paper which ſhall contain the moſt intereſting reſearches on it.

"One paper only on this queſtion has been tranſmitted to the academy, with the motto *Tamen uſque recurret*; the author of which ſeems well acquainted with the ſubject, and fully capable to examine it thoroughly. In many places he has followed the calculations of M. Schubert, publiſhed in the tenth volume of the New Tranſactions of the Imperial Academy of Peterſburgh; and has brought together, in a ſmall compaſs, a great many intereſting reſults. As the ſhortneſs of the time, however, did not permit him to give his own obſervations at ſufficient length, and as the object of the academy was to ſee removed, as far as poſſible, the difficulties with which the ſubject is ſtill attended, it has reſolved to propoſe the queſtion again for the year 1802, with a double prize; and, to place its meaning in a clearer point of view, requeſts the learned, and the author of the above-mentioned paper in particular, to attend to the follow-

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ſng conſiderations:—In regard to obſervations of the obliquity of the ecliptic, aſtronomers do not appear to be yet fully agreed. The academy wiſhes, therefore, that theſe obſervations may be carefully examined, and particularly that the following queſtion may be explained: How far can the old obſervations be employed with advantage, and how far back may we venture to go in making uſe of them?—In regard to the theory, one of its moſt important principles, without doubt, is to determine the maſſes of the perturbing planets, and eſpecially of Venus. The academy deſires that theſe may be deduced from principles independent of the obliquity of the ecliptic; and, above all, that the candidates will endeavour to diſcover how the maſs of Venus may be determined from conſidering the motion of the nodes, notwithſtanding the difficulties which the movement of the ecliptic oppoſes to this reſearch; and alſo how a ſmaller maſs aſſigned to Venus than that found by M. de la Grange, would agree with the motion of the apogeum of the ſun, as the contrary ſeems to ariſe from the formulæ of that great geometer. Laſtly, how far Herſchel's obſervations of the ſatellites of Uranus are ſufficient to determine the maſs of that planet.—The application of the general ſolution of the problem would become much more uſeful, if none of the planets were omitted to be taken into conſideration; for in that caſe the equations, which ariſe, might be compared with thoſe obtained by M. de la Grange, by his ſolution. And, on this occaſion, the queſtion propoſed by himſelf will occur, viz. Whether the maſſes of the planets, let them be what they may, provided they are poſitive, will give the equations at all times with poſitive and unequal roots?—In regard to the mean values, the *maximum* and *minimum*, the periods of the variation, &c. ſhould any direct method be diſcovered of determining them, it would be neceſſary that the author, conſidering the complex nature of analytical expreſſions, ſhould be as accurate in his calculations as poſſible; but if they are determined by repeated trials, it is

required that the author will at leaſt bring proofs *à poſteriori*, that the reſults found are ſubject to no doubt. To conclude, the academy is far from requiring that all theſe conditions ſhould be fully and completely complied with. It will, without heſitation, adjudge the prize to that paper which ſhall preſent new and ſatisfactory concluſions in regard to one article only of ſo abſtruſe a matter; for it has given ſuch extent to the queſtion, merely to open a wider field to thoſe who are fond of employing their time in aſtronomical reſearches."

BELLES LETTRES.

The claſs of the Belles Lettres has announced the following prize-queſtion for the year 1800:

On the Goths and Gothiciſm.

"1. Had the Goths, as a nation particularly diſtinguiſhed among thoſe which overturned the Roman empire in its decline, any thing peculiar either in their diſpoſition, laws, manners, and cuſtoms, or in regard to their literature and arts?

"2. Are the expreſſions *Gothic* and *Gothiciſm* any thing elſe than appellations employed at later periods to indicate the general ſtate of the arts and ſciences after the downfall of the Roman empire, and in the middle ages? And,

"3. If this be true, when did the above expreſſions begin to be introduced into common uſe?"

PHYSICS.

The Phyſical claſs has announced the following queſtions:

"The late M. Cothenius having left to the academy, of which he was a member, a legacy of a thouſand dollars, and two years intereſt due on the ſame, to be diſtributed as prizes for anſwers to queſtions in agriculture, economy, and gardening, the phyſical claſs, to which the diſpoſal of this money was entruſted, finds itſelf at preſent enabled to propoſe two queſtions, at the ſame time, in regard to the above ſubjects,

ſubjects, and to offer for the beſt paper on either a prize of a hundred dollars, to be adjudged in the year 1800.

"1. As it is a certain fact that the baſe of carbon contained in the uſual animal and vegetable manures is one of the principal articles of nouriſhment to plants, what other ſubſtances are there which may be ſubſtituted in agriculture for common manure, and may be employed with the like eſſential advantage for promoting vegetation?

"It is requeſted that thoſe perſons who propoſe anſwering this queſtion, of ſo much importance to agriculture, will ground their proofs not merely on theory alone, but in particular on experiments properly made and accurately obſerved.

"2. By what proceſs, and from what ſeeds, ſuch as thoſe of flax, poppies, the ſun-flower, or other oily ſeeds, which can be procured without great expence, or be cultivated in abundance, can an oil be extracted, to ſupply with advantage the place of olive oil, and which can be preſerved a ſufficient length of time in a good condition?—The goodneſs and quality of oil procured by expreſſion, depends, without doubt, not only on the nature of the ſeeds, but alſo on the proceſs employed to obtain it. The phyſical claſs wiſhes, therefore, that thoſe deſirous to anſwer this queſtion will attend to two circumſtances, viz. to the ſeed, that it may be of a kind fit for yielding good oil, and alſo to the proceſs beſt calculated to produce it as pure as poſſible."

Letter from his Pruſſian Majeſty to the Academy of Sciences at Berlin. Read in the Sitting of the 19th of April.

AFTER procuring the neceſſary information reſpecting the preſent ſtate of the Academy of Sciences at Berlin, it appears to me requiſite to make ſome changes, calculated to enſure to that inſtitution an exiſtence equally honourable and uſeful for the public good.

I cannot conceal from the academy, that its labours, taken in the aggregate, have always appeared to me too little

directed towards the public benefit. It has confined itſelf too much to the diſcuſſion of abſtract ſubjects, and to enriching metaphyſics with ſpeculative theories and learned diſcoveries; and has not paid ſufficient attention to direct its labours to objects of real utility, the improvement of the arts and ſciences; a ſervice by which the Academy of Paris, for example, notwithſtanding its many defects and vicious organiſation, formerly diſtinguiſhed itſelf. I could wiſh then that the Academy of Berlin would *humaniſe* itſelf more, if I may uſe the expreſſion, than it has hitherto done, by giving encouragement to efforts that contribute to the happineſs of common life, to the improvement of every thing that concerns its wants, and to its conveniences, by the conſtant application of the theory of the ſciences to things, rather than to ſpeculative meditations; that it would excite the national induſtry, which ſo often makes attempts in new channels without ſucceſs for want of the neceſſary knowledge, by furniſhing it with the principles ſuited to that art which it exerciſes; that it would endeavour to purify the different ſyſtems of moral and literary education from many vague and erroneous principles, which faſhion, and the imagination of ſome enthuſiaſtic pedagogues, have introduced, and which muſt degrade future generations; and that it would combat the prejudices and deluſions of the people, as well as the licentious and deſtructive efforts of the falſe philoſophers of the preſent day.

It is by directing the labours of the different claſſes of the academy towards objects of this nature, and towards a multitude of others, the influence of which is equally ſalutary to the ſtate and to its ſubjects, that this inſtitution can acquire the moſt glorious titles to the gratitude of the public. The talents of its members authoriſe me to entertain great hopes, and ſeem to have need only of permanent impulſe in a good internal direction. It belongs to the academy to call forth the principles of it from its own boſom. The following are a few general points, which may

ſerve

ſerve as a baſis for a new arrangement. The academy, after having maturely weighed them, will make the application of them in detail, and will draw up articles of regulation, which it will take care to lay before me to receive my approbation and ſignature.

The ancient regulations of the academy, above all that of the year 1746, will be retained, and put in force ſo far as they are not abrogated by the new.

I ſhall take care to appoint a preſident diſtinguiſhed by his rank and literary talents. His duty, above all, being to maintain the eſtabliſhed order, to concentrate and direct the labours and talents of the different members towards objects uſeful to the public and honourable to the academy, to wateh over the adminiſtration of its finanees, and be its organ with my perſon, it will be neceſſary to fix the extent and limits of his functions by an expreſs article.

The economical eommiſſion of the academy, which has hitherto ſubſiſted, ſhall be aboliſhed, and its plaee ſupplied by a directory *. The members who compoſed the commiſſion ſhall however be maintained in the enjoyment of their penſions.

The directory ſhall be formed of a preſident, the four directors † of the elaſſes, and two members, to be choſen not from the academy, but men of buſineſs, equally diſtinguiſhed by their literary merit, and eapable of preſerving the neceſſary order in the eeonomieal ſtate of the academy. I propoſe for filling theſe places, Suarez ‡ privy eounſellor of juſtice, and Borgſtede § privy eounſellor of finances, who at the ſame time will be elected members of the aeademy.

Every thing which relates to the general direction of the academy as a body, to the maintenance of internal order,

* The ex-miniſter J. Ch. Wöllner aſſiſted at the ſitting of March 29, as chief of the economic commiſſion for the laſt time.

† H. B. Merian, J. Bernoulli, F. K. Achard, and Ch. G. Selle.

‡ Died on the 14th May 1798.

§ Inſtalled in the ſitting of May 3.

the management of its finances, and, above all, the direction of the academy towards objects of public utility, ſhall be the province of the directory. Its deliberations ſhall be decided by a plurality of votes. Each member ſhall have one, and the preſident two.

The influence and rights of the directors in their claſſes ſeem alſo to require to be more particularly defined and regulated; and the academy will take care to make proviſion on this head by an article in its regulations.

The members of the academy ſhall be either honorary, or ordinary. The former, not being properly obliged to engage in its labours, cannot enjoy any of the lucrative advantages of the academy, except in regard to medals in caſe they are preſent. The ordinary members ſhall be divided as hitherto into four claſſes.

Each claſs ſhall be compoſed of a director and ſix members, which will form a whole of 24 academicians, beſides the members of the directory. It will be proper to adhere in future to that number, and not to admit new members but when there are vacancies. There can therefore be no new election but when the members of each claſs are below ſix in number. As at preſent there are claſſes which have more, it may perhaps be poſſible to transfer ſome of them to the claſſes which have not the fixed number, or to thoſe which have fewer ſupernumeraries, in order to eſtabliſh a kind of equality between the claſſes, and by theſe means to approach as near as poſſible to the order to be obſerved in future. In the laſt place, the right of electing its members ſhall be preſerved to the academy; and this election ſhall be determined by a plurality of voices of all the members. I, however, reſerve to myſelf that of confirming or rejecting.

The large public library at Berlin, as well as the collection of natural curioſities, ſhall be united in future with the academy, and entruſted to its direction. It will be therefore neceſſary to determine, by a regulating article, the arrangements to be made in that reſpect; and as it will be requiſite

that

that the principal librarian be an academician, the academy will have the leſs heſitation to admit Dr. Bieſter * among its members, as his knowledge and literary merit have already enſured him the ſuffrages of the public.

To conclude, though I am diſpoſed to preſerve to the academy the enjoyment and adminiſtration of its funds and revenues, I, however, reſerve to myſelf the right of deciding more particularly on this ſubject, after the new ſtate of its economy for the next year ſhall have been preſented to me for ſignature. In regard to that of the current year, which accompanied the letter of the 28th, it is herewith returned.

FREDERICK WILLIAM.

1798. Received April 11th.

Electoral Academy of the Uſeful Sciences at Erfort.

IN the ſitting of January 2d, 1798, a paper was read by F. von Dalberg, *On the gamut and muſical ſyſtem of the ancient Hindoos.* The author took as the grounds of his diſſertation a paper of the late Sir William Jones, publiſhed in the *Aſiatic Reſearches*, and combined with it what he had learned during his reſidence in Italy and England, partly from the converſation of Richard Johnſon, the friend of Sir William Jones, and of others; and partly from manuſcripts. He brought as proofs, that the Hindoos were acquainted with the art of noting muſic, not only ſeveral of their muſical pieces and a figure of their principal inſtrument called *vina*, but alſo their mythology, in which their principal tones and notes are perſonified as deities, which have been hitherto unknown. M. von Dalberg obtained figures of theſe, collected by Johnſon in India, in order that they might be engraved. In the writings of the Hindoos every thing is perſonified. According to their ideas, muſic, like all the other fine arts, came from heaven. They conſider muſical tones as real beings which marry other tones; and hence

* Inſtalled in the Sitting of April 26.

ariſes

arifes the affinity of the gamut or mufical fcale, &c. This paper will be printed.

At the fame time was read a paper tranfmitted to the academy by Major George Vega, entitled, "Mathematical conſiderations on the directions of gravity, the length of a pendulum that fwings ſeconds, the method of determining the latitude from the true elevation of the pole, and the length of a degree of the meridian in different latitudes, by means of a ſolid globe revolving with an equable motion round an immoveable axis; as alſo on the form of the ſurface of the water in a ſtate of equilibrium on ſuch a globe; and on the neceſſity of correcting the apparent latitudes or altitudes of the pole, in order to obtain the real latitudes, both by calculating the diſtances of places from their longitude and latitude, and alſo by delineations made on a portion of ſuch a globe, upon any projection at pleaſure, regard being had to the ſphercidal form of our earth."

M. Burckhardt tranſmitted to the academy a paper under the following title, which was alſo read: "On the employment of combinatory analyſis for determining the ſines, tangents, &c. of the ſum of ſeveral angles, when the ſines, tangents, &c. of ſingle angles are given."

A paper was tranſmitted likewiſe from M. Topfer, on the doctrine of combination, and its application to analyſis.

In the ſitting of February 3, was read a paper on the Ηθικα Μεγαλα, tranſmitted by Dr. W. G. Tennemann at Jena. In this paper, which will appear in the Tranſactions of the Academy, the author endeavours to ſhew, that the ſo called *magna ethica* of Ariſtotle are an extract from thoſe of Nicomachus.

Some remarks were then read by M. Zizman on the cultivation and employment of the monarda (oſwego-tea) as a ſpice. The author found the *monarda fiſtuloſa* and *didyma* LINN. to be plants, the leaves, flowers, and, in particular, the ſeeds of which might be ſubſtituted for many kinds

kinds of ſpiceries. The leaves, beſides other uſes, may be employed as tea; the flowers improve the taſte of brandy, and give it a flavour like that of peaches, &c. The moſt important part, however, is the ſeeds. The author deſcribed the method of cultivating this plant, and alſo of preſerving it.

Profeſſor Trommſdorf at Erfort tranſmitted a paper entitled, "Collections towards a more accurate knowledge of the nature of Strontian earth."

FRANCE.

IN the public ſitting of the National Inſtitute Vendemiaire 15, after an account had been read, by the ſecretary of each claſs, of the labours of the Inſtitute, C. Camus gave an account of the labours undertaken by the Inſtitute as a body, or executed under its direction.

C. Cuvier read a learned though ſimple and clear memoir on an animal, bones of which are found in the plaſter ſtone in the neighbourhood of Paris. He began by explaining how a ſingle bone, eſpecially if it belong to the head, or an articulation, is ſufficient to make known very nearly the whole ſtructure of an animal, reſpecting which naturaliſts have no other knowledge. A tooth, for example, ſaid he, indicates whether an animal is carnivorous, or feeds upon vegetables. If it is carnivorous, its ſtomach is formed in a certain manner; the animal unites with the means of attack and defence the agility neceſſary to reach its prey: its feet muſt have a certain form: in a word, it has all the habits common to that kind of animals, &c. After explaining this principle, C. Cuvier applied it to the bones found in the quarries of plaſter ſtone, and which are ſo abundant that not a decade paſſes but the workmen break ſeveral of them in digging out the ſtones. He examined a great many of theſe bones, and thinks himſelf authoriſed to aſſert, that they belong to a graminivorous animal ſimilar to the camel, not now exiſting alive in any known country.

C. Dé-

C. Déſeſſarts produced ſome new facts in ſupport of his ſyſtem, that the natural ſmall-pox may be rendered much more favourable by the proper uſe of ſome mercurial applications. He, however, expreſſed a wiſh of ſeeing inoculation become more general, and that this cruel malady may be altogether deſtroyed by weakening its malignity.

C. Bougainville, diſtinguiſhed both as a writer and a navigator, read the firſt part of his Hiſtorical Eſſay on Ancient and Modern Navigation in high northern latitudes.

C. Langles read a memoir on the language and literature of the Arabs.

C. Lacepede ſhewed, from the induſtry diſplayed by birds in the ſtructure of their neſts, the meaſure of the different degrees of their inſtinct, and of the attachment they have to their females and young.

A friend read for C. Rœderer an explanation of the motives which induced his claſs to propoſe as the ſubject of a prize, "An examination of the inſtitutions beſt calculated to lay a foundation for morality among nations."

C. Fourcroy gave the reſult of ſome experiments which he made in concert with C. Vauquelin, to analiſe the ſtone formed in the human bladder. Theſe two chemiſts think it poſſible to diſſolve in the living ſubject this fatal accumulation, which occaſions the moſt acute pains; and they hope that the reſult of their labours will render unneceſſary in moſt caſes the dangerous operation of cutting *. This will form a new benefit of chemiſtry, already ſo uſeful in its application to the arts of induſtry.

The ſitting was terminated by C. Ducis reading his *Epitre à Vien* on the great revolution which has taken place in painting ſince the middle of the preſent century, a revolution which he chiefly aſcribed to the precepts and examples of Vien.

* Water impregnated with carbonic acid gas, and drank for ſome time as a common beverage, has been found to diſſolve the ſtone. EDIT.

MISCEL-

MISCELLANEOUS.

BOTANY.

THE plants brought from America and the Weſt Indies by Captain Baudin, and contained in a hundred and fifty boxes, are in a high ſtate of vegetation. Several of them have produced flowers, ſuch as a ſpecies of bignonia, (*bignonia pentaphylla* L.) Its flowers, of a fleſh colour, are larger than thoſe of the *catalpa*, and have the ſame form. A kind of *tournefortia*, which appears new; an *euphorbia*, the leaves of which are of an agreeable green colour, and have a little reſemblance to thoſe of the ſumach; and, laſtly, the *jatropha goſſipifolia* L. The laſt has begun to exhibit fruit.

Among this great variety of foreign trees there are ſome which it is hoped may be naturaliſed in the ſouthern departments of France, ſuch as the alligator pear (*laurus perſea* L), a tree which the Spaniards tranſplanted from the New World to the kingdom of Valencia, where it produces fruit. Its fruit is pulpy, of the ſize of a very large pear, and is eat with ſalt and pepper. The papaw tree (*carica papaya* L). There are alſo ſeveral alimentary plants, ſuch as two kinds of yams, *dioſcorea alata* and *dioſcorea aculeata*, with the white and red potatoe, *convolvulus batatas*, &c.

Among the curious plants are remarked in particular a ſpecies of fern, the ſtem of which is three feet and a half in height and three inches in diameter, and is at preſent terminated by two leaves more than two feet in length, and having very fine and equal diviſions.

The four kinds of palms begin to ſhoot, and above all the cabbage palm. Theſe majeſtic trees ſeem likely to become naturaliſed to the climate of France.

A SINGULAR PHENOMENON IN REGARD TO CREAM.

The following ſingular phenomenon is announced in the *Journal de Phyſique, Thermidor 6th*, 1798, by Cit. de Serain *officier de ſanté* at Saintes.—"This ſummer I was witneſs

neſs to an extraordinary fact, to me totally new, and which, in my opinion, cannot eaſily be accounted for. One day when ſome people in this neighbourhood were preparing to churn butter, they were aſtoniſhed to find all the cream of a fine Pruſſian-blue colour. The caſeous part was only blueiſh. Every attempt to diſcover the cauſe of this extraordinary colour was fruitleſs, though the cream exhibited the ſame appearance for nearly three months. It cannot be aſcribed to the veſſels in which the milk was generally preſerved, as they were always kept perfectly clean, and covered with fir-boards. The cows were in exceeding good health, and fed on meadows on which they had grazed for ſeveral years. This milk was uſed as food without any hurt enſuing, and it betrayed no particular taſte; but the cream and caſeous parts were thrown away, as they inſpired ſome dread. The cream gradually changed its colour; but this could not be aſcribed to the means employed during the continuance of the phenomenon, means indeed ſo ridiculous that I do not think it worth while to detail them.

"In the Ephemerides of the Curious of Nature, Dec. 2d, 1688, we find inſtances of milk being coloured green, black, red, and yellow; but I am acquainted with no obſervation ſimilar to that above mentioned."

MEDICINE, SURGERY, &c.

A ſmall work, conſiſting of 30 pages octavo, has been lately printed at Vienna, and diſtributed gratis: entitled, "An account of the ſimple means employed in the Hoſpital of St. Anthony at Smyrna to cure the Plague, and preſerve People from its infection. By Leopold Count von Berchtold, knight of the military order of St. Stephen of Tuſcany." Mr. Baldwin, the Britiſh conſul at Cairo, having found by repeated experiments that rubbing the whole body with warm oil was the ſureſt means to cure the plague, as well as to guard againſt its infection; a Franciſcan clergyman, overſeer of the Hoſpital of St. Anthony, had, in conſequence of this information, employed the ſame means

means with the happieſt effects. To make known this diſcovery, of ſo much importance to thoſe nations that carry on trade in the Levant, and with the coaſt of Barbary, Count Berchtold reſolved to publiſh the proceſs; of which 3000 copies have been printed in the German, as many in the Italian, and 6000 in the Turkiſh language. The work contains an account of the method of uſing the oil, with ſome obſervations on the regimen proper for patients attacked by this diſeaſe; and an Appendix, containing certificates of cures already performed, and a liſt of the places where the above method has been practiſed with ſucceſs.

In a little tract juſt publiſhed by Dr. Tazwell (ſecretary to Mr. Geary, the late American miniſter,) at Paris, a new method is announced of employing the nitrous acid in the ſyphilis, which we are informed has been uſed with great ſucceſs at Paris. As it is there conceived to be an improvement upon the preſent modes of adminiſtering oxygen in ſyphilitic ulcers, &c. and as the practice with the nitrous acid is at preſent a ſubject of ſome controverſy, it may not be unworthy of the attention of our medical readers. Dr. Tazwell's formula is as follows:

Oxygenated Ointment (invented by Alyon).

℞. Take hogs-lard lib. j. melt it over a ſlow fire, and then add of nitric acid (of 32 degrees) ʒ ij. Keep carefully ſtirring the mixture over the fire with a glaſs rod till it begins to boil, and then ſet it by to cool.

It is of great benefit in caſes of ſyphilitic ulcers, herpes, and pſora.

The author adds the following note: "This preſcription was communicated to me in the month of Prairial, laſt year, by my worthy friend the inventor, who has ſince publiſhed it in his work entitled *Eſſai ſur les proprietes medicinales de l'oxygen*, which has juſt appeared.

M. Herbolt, an eminent accoucheur at Copenhagen, ſays he, has made a diſcovery highly intereſting to humanity.

Repeated

Repeated experiments have convinced him that one of the moſt frequent cauſes of the apparent death of ſo many new-born children, ariſes from the trachea being filled with water; and that they might in general be ſaved, by giving them ſuch a poſition that the water ſhould run out. Of thirteen children conſidered as dead, and which were committed to his care, twelve were reſtored to life by the above ſimple means.

The above is announced in a foreign Journal, but appears evidently to be founded upon ſome miſtake. By the experiments of Dr. Goodwyn it appears impoſſible that water can of itſelf in any caſe enter the trachea: even drowned people are not ſuffocated by this means, but by the mere excluſion of the oxygen of the atmoſphere neceſſary to ſupport animal life. In thoſe experiments in which water has been forced into the trachea, it has always been abſorbed, if the animal was ſuffered to live.

In our laſt Number, (p. 426.) by a miſtake of the Printer, the following obſervation was omitted after the article on the production of cold by compreſſed air:

The explanation given of the phenomena in the Journal de Phyſique, from which we extracted this article, is inadmiſſible in the preſent inſtance; for it has been proved by Dr. Darwin's experiments, publiſhed in the Philoſophical Tranſactions, that cold is produced by compreſſing air even where water is not preſent, and that the heat of the air is abſolutely ſqueezed out by condenſation, and paſſes out through the ſubſtance of the condenſer. The compreſſed air therefore, when allowed to eſcape, muſt rob water, or any ſubſtance with which it comes in contact, of a portion of its heat.

THE

PHILOSOPHICAL MAGAZINE.

NOVEMBER 1798.

I. *Description of the* Equus Hemionus *or* Dshiggetai *of the Eastern Deserts of Middle Asia**. *By Professor* PALLAS. *From* Neue Nordische Beytrage, *Vol. II.*

NATURALISTS hitherto have distinguished only three species of the genus horse, or animals which have not cloven hoofs, viz. the horse and the ass, which are both found wild in the desert regions of Asia, and the beautiful striped zebra †, which, with so many other singular animals, is peculiar to Africa. I have had an opportunity, however, of becoming acquainted with a fourth species (Plate V. fig. 2.) the Mogul name of which has been long known in Europe. But respecting this animal zoologists as yet have been under great doubts; because the accounts given of it were either defective or little to be depended on, as might readily be ex-

* See Professor Pallas's Travels, in the original, p. 217; and *Nova Comment. Petropol.* tom. xix. p. 394. tab. 7. Equus Hemionus.

† It has been proved by Dr. Sparmann that those animals, before considered by naturalists as the males of the zebra, are a particular species, which rove about in herds; and, consequently, form the fifth kind of *solidungula*. They are called, at the Cape, by the Hottentot name of *quagga*.

pected in regard to an animal so swift, and at the same time so shy, that it carefully shuns all inhabited districts, and is scarcely ever to be found but in the mountainous deserts of Eastern Tartary, between Siberia, India, and China. I have thought proper to give to this animal the Greek name *hemionos*; which the ancient Greeks applied not only to the mule, but also to a wild timid species, capable of propagating, similar in form to the mule, which were, in some measure, known to them, and which probably were the same as the *dshiggetai*. Aristotle is the first author who mentions this animal, in the sixth book of his Natural History, where he distinguishes it very clearly from that produced between the horse and the ass, or the ass and the mare; and he gives it the name of *hemionos*, because this species, which were then found in a wild state in Syria, and exceedingly timid, had as great a resemblance to the mule as the wild ass has to the tame *. This supposed species of wild half-ass, nine of which were brought to Phrygia, under the reign of Pharnaces, where they were long preserved, and of which three were alive at the time when Aristotle wrote, propagated regularly, and were therefore a peculiar species and not a bastard race. The same author mentions this circumstance expressly, and also Syria the native country of the animal, in the twenty-fourth chapter †. This animal, admitted by Theophrastus, is mentioned likewise by Pliny ‡, who gives Theophrastus as his authority; and it is probable also that Ælian alludes to it in a passage where he says: "In India there are herds of wild horses and wild asses; and the wild

* Εισι δὲ εν Συρία οἱ καλούμενοι ἡμίονοι, ετερον γενος των εκ συνδυασμοῦ γινομενων ἵππου και ὄνου, ὁμοιοι δε την ὄψιν, ὥσπερ καὶ οἱ ἄγριοι ὄνοι προς τας ἡμέρους, απο τινος ὁμοιότητος λεχθεντες εἰσὶν, ὥσπερ οἱ ὄνοι αγριοι και ἡμίονοι, την ταχυτητα διαφέροντες, αὗται αἱ ἡμιονοι γεννῶσιν ἐξ ἀλλήλων· σημεῖον δὲ ἦλθον γαρ τινες εις Φρυγιαν επι Φαρνακου του Φαρναβαζου πατερος, και διαμενουσιν ἔτι· εἰσι δε νυν μεν τρεῖς, το παλαιον δ' ἐννέα ἦσαν ὡς φασιν. Aristot. Hist. Animal. lib. vi. cap. 36.

† Αἱ δε εν τη Συρία τη υπερ Φοινίκης ἡμίονοι καὶ ὀχευονται και τίκτουσιν. Lib. vi. cap. 24.

‡ Hist. Nat. lib. viii. cap. 44.

mares

mares copulate readily with the latter, and produce mules of a red fox colour, which are exceedingly ſwift. They are very untractable; and when caught in gins are ſometimes carried to the king of the Praſians. Thoſe of two years old, or under, can be tamed; but the old ones are as wild as the fierceſt of the carnivorous animals *."

For the earlieſt account of the exiſtence of this ſpecies of the horſe in modern times, we are indebted to the diligent Meſſerſchmidt, who from the year 1720 to 1726 travelled through Siberia, by command of Peter I. for the purpoſe of making diſcoveries in natural hiſtory. Beſides this traveller and Gmelin, who explored the ſame diſtricts twenty years after, no one before me has had an opportunity of examining this animal with the eye of a naturaliſt. Meſſerſchmidt diſtinguiſhes the dſhiggetai, very properly, from the horſe and the aſs; and notices it in his *Xenium Iſidis Sibiricæ*, or Catalogue of the natural productions of Siberia; the manuſcript of which is ſtill to be ſeen in the academy of Peterſburgh †, under the name of *Mulus dauuricus fœcundus Ariſtotelis, Cappadocicus Ereſii*, which name was employed in the printed catalogue of the collection of natural curioſities at Peterſburgh, where a ſtuffed ſkin of it, deſtroyed afterwards by a fire, was preſerved, and which Buffon has improperly taken for an obſcure definition of the *onager* or proper wild

* Εν Ινδοις ἵππων τε ἀγριων καὶ ὄνων τοιούτων εἰσὶν ἀγέλαι· ἐκῶν αναβαινοντων ονων τας ἱππους ὑπομένειν ἐκείνας λέγασι, καὶ ἥδεσθαι τῇ μίξει, καὶ τίκτειν ἡμιονους πυρσὸς την χρόαν καὶ ἄγαν δρομικοὺς, δυσλόφους δὲ καὶ γαργαλεῖς ἄλλως· ποδαγρα ς δὲ τουτους ἁιρῦσι εἶτα ἀναγεσθαι τῷ τῶν Πραισίων βασιλεῖ φασι· καὶ διετεῖς μὲν ἐαλωκότας μὴ ἀναίνεσθαι τὴν πώλευσιν, πρεσβύτατους δὲ μὴ διαφέρειν τῶν παρχάρων θηρίων καὶ σαρκοφάγων μηδε ἕν. Ælian. de Animal. lib. xvi. cap. 9.

† *Catalogus Muſæi Petropolit.* tom. i. par. 1. p. 335. In the above-mentioned *Xenium Iſidis*, Meſſerſchmidt gives to this animal, beſides the Mogul name, which he writes *tzigithai*, the Tangut appellation *kſching*, and the Indian *kitſchara* or *dſhengli-kitſcharah*. But when he wiſhes to apply to it alſo the *parad* of the Bible, the *hamar iwaſchi* of the Arabians, the *thar-kurah* of the Perſians, and the *kolan* or *kulann* of the Tartars, he evidently confounds the *onager* or wild aſs with the *dſhiggetai*.

aſs*. The deſcription given of this animal by Meſſerſchmidt, which, as he ſays in his journal, was made from three that had been ſhot, is all, except what is inſerted in the ſaid journal, unfortunately for the farther oſteology of the *dſhiggetai*, now loſt; and this deſcription, as well as the journal, has never yet been printed.

Gmelin, when in Dauuria, did every thing in his power by ſending out hunting parties to procure one of theſe animals, but without ſucceſs†. Afterwards, however, by the care of his friend and fellow-traveller Müller, he had an opportunity, in returning from the Lena, to draw up a deſcription of this animal at Irkuzk; but this deſcription, which ſtill lies in manuſcript among other papers, is ſo unintelligible, that it is almoſt uſeleſs for the naturaliſts of the preſent day. Zoology, in general, was not Müller's principal talent; and however much ſervice he may have rendered to botany, his deſcriptions of animals, as may be ſeen by the ſpecimens publiſhed, are exceedingly defective. During my travels through Siberia, I conſidered this animal as almoſt unknown; for the Jeſuit miſſionaries, who on various occaſions have made excurſions from China into the Mogul provinces, ſay little more of it, in the accounts of their travels, than merely to mention its name‡.

I was at great pains therefore, and ſpared no expence during my travels, particularly when in the neighbourhood of the ſolitary deſerts on the borders of the Ruſſian empire, to procure the *onager* or wild aſs, as well as the *dſhiggetai*, and to acquire a more accurate knowledge of theſe animals. All my exertions, however, were attended with no ſucceſs, till I arrived, in the ſpring of the year 1772, in the moſt diſtant diſtrict of Dauuria, which begins between the rivers

* Hiſt. Nat. tom. xxiv. p. 6, note.

† *Reiſe durch Sibirien*, part 2, p. 107, where a ſhort but accurate deſcription of this animal is inſerted from the information of the Moguls.

‡ Wild mules are mentioned under the Chineſe name of *ye-lo-tſee*, which expreſſes that appellation, by Du Halde in his Deſcription of China.

Onon

Onon and Argun, and extends ſouthwards towards the country of the Moguls and the great deſert of Gobi. In theſe waſtes, which are very thinly inhabited, where Meſſerſchmidt and Gmelin became acquainted with the dſhiggetai, I was ſo fortunate as to obtain a deſcription of this rare animal. The neighbourhood of the river Argun is the part within the boundaries of Siberia where theſe animals are ſtill to be found; for they have long ſince retired from the reſt of Dauuria, where they once abounded, to the Mogul deſerts, on account of the increaſe of population in the former. They frequent, in general, the whole extent of the deſert of Gobi, as far as the boundaries of Thibet and India; but they are moſt abundant in the dry plains near the lake Tarei, which afford plenty of herbs and pools of ſalt water, and in the hilly diſtricts of Abagaitu. The reſt of Dauuria abounds with rocky and high mountains covered with ſnow, which, as well as diſtricts covered with wood, theſe animals never frequent.

Theſe animals were found formerly in herds in the diſtricts near the Argun. At preſent, however, none are ſeen there but ſingle ones which have ſtrayed, or ſmall diſperſed herds, except when great drought takes place in the Mogul diſtricts, and drives them northwards. In the country of the Moguls, on the other hand, and particularly in the above-mentioned deſert of Gobi, they are at all times collected into large herds; and are well known to the Moguls and the Tunguſians under the name of *dſhiggetai*, which ſignifies *long-eared*. They muſt alſo extend themſelves to the country of Soongarey, becauſe they were known to the Soongarey Calmucs, whom I had an opportunity of converſing with on the Wolga, under the above name, as an animal totally different from the wild aſs, which they call *kulan*, and which by the wild Coſſacs is called *takia*. But in the weſtern part of Great Tartary it appears, that the dſhiggetai is never found; for the Kirgiſians are acquainted with no middle animal between the wild horſe and their kulan or

wild aſs. It is not improbable that the great Altai mountains form the boundaries of its native country on the eaſt towards the extenſive ridges of Thibet and India; for the accounts of travellers who have ſpoken of wild aſſes in Syria and Perſia, are not accurate enough to enable us to judge whether the dſhiggetai is found in theſe countries, which I very much doubt.

The dſhiggetai ſeeks the open dry plains and the declivities of mountains covered with good nouriſhing herbs and graſs, as is the caſe with all Dauuria and the country of the Moguls, which are full of mountains. It is ſaid that they ſeldom approach the water, and that they can continue a long time without drinking; which, to an animal deſtined to live in deſerts, where often for a hundred werſts there is not to be found a drop of drinkable water, is a very neceſſary property.

The onager and the wild horſe have yielded to the perſeverance and courage employed by man to tame them, and render them fit for being draught animals, and animals of burthen; but the dſhiggetai, like the African zebra*, has never yet been tamed, though the Moguls, who are born horſemen and ſhepherds, have often tried to breed foals of this ſpecies which they have caught. I am however of opinion, that we ought not to abandon all hope of rendering the dſhiggetai a domeſtic animal, if ſerious attempts were made in Dauuria on foals when caught and ſhut up in proper enclоſures, which no wandering people have an opportunity of doing. Should ſuch an attempt ſucceed, mankind would procure in this animal, if broke for riding, not only the fleeteſt and ſwifteſt hunter, but give to the eaſtern part of Aſia, India and China, in particular, where the common horſe, as is well known, does not thrive, a far more uſeful draught animal, becauſe theſe regions are as it were its na-

* The opinion entertained of the impoſſibility to tame the zebra has been however contradicted by the one reared at Liſbon and taught to draw in a carriage. The caſe perhaps will one day be the ſame with the dſhiggetai.

tive

tive country. That the fruitleſs attempts made by the Moguls do not prove the intractableneſs of the dſhiggetai appears probable from this circumſtance, that the Kirgiſians aſſert the ſame thing, on account of the fruitleſs attempts they have made in regard to the onagers or wild aſſes, which wander through their territories in thouſands; though the people of the eaſt, who live in ſettled habitations, have not only made theſe animals domeſticated ſince time immemorial, but, as we ſee by Varro, Columella, Pliny, and the teſtimony of other authors, employed with great advantage, inſtead of mules, wild aſſes, which they caught and tamed. Varro ſays expreſsly *, that the wild aſs is exceedingly proper to be uſed inſtead of the mule, becauſe it can be eaſily tamed, and never returns again to its wild condition.

Hitherto the dſhiggetai has been only an animal of the chace for the Moguls and Tunguſians, who conſider its fleſh as their greateſt dainty, and employ the ſkin to make boots. It is, however, difficult to kill it; for, on account of the keenneſs of its ſight and the acuteneſs of its ſmell, by the latter of which, if to the leeward, it can diſcover a man at the diſtance of ſeveral werſts, it ſeldom lets the hunters get within ſhot. When running, it is impoſſible for the fleeteſt horſe to overtake it; and therefore it is ſeldom caught during the hunting excurſions of the Moguls, which they call *oblaw*, but muſt be ſhot, by lying in wait for it; which can be beſt done in the neighbourhood of the ſtreams or pools to which it repairs to drink, or of the ſpots where it comes to lick ſalt. The Moguls, however, are ſaid to have remarked, that during rainy and ſtormy weather the dſhiggetai becomes, as it were, ſtupid, and neither ſees nor ſmells the hunters ſo well as at other times. The ſtallions, which conduct herds more or leſs numerous of females and young ones of from one to two years old, are exceedingly vigilant; keep their females together with the moſt jealous care; drive from the herd the young ſtallions which begin to ſhow a deſire for the females;

* De Re Ruſt. lib. ii. cap. 6.

and are always on the watch to guard againſt being ſurpriſed. When they obſerve any thing uncommon at a diſtance, the male immediately ſprings forwards; endeavours, by making a tour, to approach ſo near the object as to diſcover the danger, and for that purpoſe advances two or three times towards the hunters, who lie cloſe to the ground on their bellies. On ſuch occaſions ſome of them are ſhot; the herd then diſperſe, and for ſome time afford good ſport to the huntſmen. If the male, however, obſerves the danger, he makes the herd which he left behind him betake themſelves to flight with incredible velocity. The Moguls ſpeak of this velocity with aſtoniſhment; and, in general, the dſhiggetai is conſidered as the ſwifteſt of all the wild animals of their country: for this reaſon the people of Thibet aſſign it, inſtead of a riding horſe, to *chammo*, their god of fire and of war.

The dſhiggetai always carries its head erect, as repreſented in the plate; when running, throws it entirely back, in order that it may ſee better behind it; and bears its tail erect. It has a particular kind of neighing, which is louder and ſhriller than that of a horſe. The herds by which old males are attended conſiſt often of more than twenty males and females; though, in general, they are leſs numerous, as many males have no more than five or ten females. Young males driven from old herds generally follow them at a diſtance, till they have enticed from them one or more females, or have found ſome that have ſtrayed from other herds, and thus procure to themſelves a few followers. At the covering ſeaſon the old males ſeparate from their herds the young females not yet hot. The Moguls are ſaid to have remarked alſo, that theſe half-aſſes ſometimes entice away mares from the troops of tame horſes which wander about in theſe diſtricts, and add them to their ſeraglio. Of this, however, I am not fully convinced; though, on account of the great ſimilarity and likeneſs in regard to ſize which the dſhiggetai has with the Mogul horſes, procreation muſt be much eaſier

between

between them than between the horſe and the aſs, and muſt be always more fruitful; ſo that if the horſe were not known in his wild ſtate, and if characteriſing marks of a particular ſpecies were not viſible in the dſhiggetai, on account of this great ſimilarity, we might with much more reaſon conſider the dſhiggetai for the wild ſtock of our horſe than the younger Gmelin conſidered the wild horſe as the common ſtock of the horſe and aſs *, which, however, beſides the mouſe colour obſerved by Gmelin, which is not always uniform, has nothing in common with the aſs.

[To be concluded in the next Number.]

II. *Obſervations on Animal Fat and the Cauſes of Corpulency. By Dr.* COINDET. *From the* Journal de Phyſique, Vendemiaire An. 7.

ANIMAL fat examined by the microſcope ſeems to exhibit yelloviſh veſicles, formed of a very thin and tranſparent pellicle, which contain an oily fluid. No pores can be obſerved in it, and no perſon but Malpighi has been able to diſcover what are called its adipoſe ducts *(ductus adipoſi)*. In certain caſes, however, fat is abſorbed, and in general it ſeems to undergo conſtant changes: the ſkins of the negroes after violent exerciſe exhale an oily odour.

The veſicles of fat are different in different animals. Wolf of Peterſburgh obſerved that the fat of a pullet is contained in veſicles ſmaller than thoſe of any other animal: thoſe of the gooſe are larger, and ranged with more regularity. The next in order are thoſe of man; but, according to Wolf, the moſt conſiderable are thoſe of the hog. Theſe veſicles are contained in the interſtices or ſmall ſpaces in the cellular

* See Sam. Gottl. Gmelin's *Reiſe durch Ruſſland*, part i. p. 47. Compare, alſo, what I have ſaid of wild horſes in my Travels, part i. p. 211, 272; part ii. p. 642, and part iii. p. 509.

tiſſue,

tissue, but are not found equally dispersed throughout every part of the body. Fat is found in a pretty considerable quantity in certain cavities of the body, as in the orbits of the eyes, near the reins, in the epiploon, sometimes at the point of the heart, &c. In man there is none about the lungs, the penis, or the brain. Some is contained in the last-mentioned organ in the cold-blooded animals: in the sea-dog, the ray, and many other fishes, the fat is mixed with the parenchyme of the liver, to which it gives considerable bulk: in birds and animals that feed on vegetable substances, it is found around the reins. The fat of fishes is more liquid than that of carnivorous quadrupeds: that of frugivorous animals has more consistence than the fat of others.

The quantity of fat varies much according to the different classes of animals; and, if we compare the quantity of fat with the bulk of the body, it will be found that fishes exhibit the largest proportion; then amphibious animals, and then the frugivorous. The carnivorous present the least. But these are only general observations, which are subject to many exceptions.

All the uses of fat are not yet known; but we know that in some cases it becomes exhausted, and supplies the place of aliment. Thus, animals which remain in a state of torpor for several months without taking nourishment, lose their plump appearance; from which it would seem that this is a resource provided for them by nature. May it not perhaps serve to preserve animal heat? The circumstances which contribute to its formation are still more obscure, and have given rise to many ingenious ideas dignified with the name of hypotheses. One of the most probable is that of Dr. Beddoes*, which appears to me to clear up many facts

* Dr. Coindet has done little more than abridge from Dr. Beddoes's *Observations on Calculus*, &c. though he prefixes his own name to this paper, and thus casually introduces the name of the latter, as if his writings only tended to confirm, when, in fact, they are the foundation of the doctrines here brought forward. Edit.

hitherto

hitherto inexplicable, though it is ſubject to many exceptions, as every hypotheſis muſt be. The foundation on which it reſts is, that whenever there is a certain diminution of oxygen in the animal ſyſtem, fat will be produced. The following obſervations ſeem to ſupport this aſſertion: The chemical analyſis of fat ſhows that ſix parts of it contain near five of carbon and one of hydrogen, and ſome ſebacic acid. The fat parts of animals differ from the fleſhy parts only in this, that the latter contain more oxygen and azot. By this is explained the change of muſcles into a ſubſtance like ſpermaceti, as profeſſor Fourcroy remarked in the burying ground of the Innocents at Paris. It has been obſerved alſo, that the fat augments at the expence of the muſcles in the living body, and *vice verſa**.

This want of oxygen conſidered as a cauſe of corpulency is indicated by the analogy which exiſts between obeſity and the ſea ſcurvy, which ſeems to be owing only to a gradual abſtraction of a part of the oxygen in the ſyſtem. The ſea ſcurvy is never announced by meagreneſs; on the contrary, a fullneſs of the habit is the firſt ſymptom of that malady. Dr. Trotter obſerves, that when a negro grows rapidly corpulent, he does not fail to be attacked by the ſcurvy; from which, to make uſe of a compariſon of Dr. Beddoes, it appears that corpulency is to the ſcurvy what cachexy is to the dropſy. All the ſymptoms of the ſcurvy prove that it ariſes from a privation of oxygen: thus the ſurface of the body is covered with livid ſpots, the arterial blood is very little florid, and, after death, the left auricle is found filled with venous blood, which Dr. Goodwin found in animals that had been deprived of life by oxygen. Dr. Lind ſays, that when death has been ſudden, and that no effuſion is found in the cavities of the body, the auricles and the ventricles are filled with blood, and eſpecially the left ſide of the

* Very fat hogs have ſcarcely muſcle enough to perform locomotion. EDIT.

heart;

heart; which is a very remarkable circumſtance, ſince that ſide rarely contains much blood after death.

According to the experiments of Sauſſure and Humboldt, it appears that the atmoſphere of the high mountains contains leſs oxygen than that of the lower regions; and this explains why la Condamine was attacked with ſcorbutic ſymptoms on the ſummit of Pinchina. Much ſleep and inactivity are powerful cauſes of corpulency. In that ſtate reſpiration is leſs frequent, a ſmaller quantity of oxygen is abſorbed, and the abſorption of fat is diminiſhed, while the ſecretion of it is continually taking place. Leanneſs is produced by a contrary ſtate, that is to ſay, by the effect of exerciſe: more oxygen is then introduced into the ſyſtem, and the fat is abſorbed in common with the other fluids.

People in the country know very well from experience, that when they wiſh to fatten poultry they muſt keep them in darkneſs, and mix with their food ſubſtances proper for prolonging their ſleep, ſuch as tares or ſpiritous liquors *. An obſervation which appears curious is, that the age when the ſecretion of fat is moſt conſiderable is towards the fortieth year; a time when the arterial ſyſtem ceaſes to act ſuch a conſpicuous part in the animal economy, either becauſe it is then oſſified, or in part obliterated, while the venous ſyſtem, becoming more and more developed, ſeems to acquire that influence ſo conſiderable in old age. Do arteries the action of which is viſibly diminiſhed, furniſh at that period leſs oxygen to the ſyſtem; and may not that be the cauſe of the corpulency of middle-aged people?

It may be objected that children, whoſe venous ſyſtem is not yet developed, have however a remarkable plumpneſs. But this difficulty has been reſolved in the following manner: The venous blood when it arrives at the lungs undergoes there chemical changes, too well known to require to be

* In Portugal, fowls are not only kept in the dark, but their feet are faſtened to the floor, ſo as totally to prevent their walking. EDIT.

here

here particulariſed. It may happen that, according as reſpiration is more or leſs perfect, the blood may loſe a greater or leſs quantity of carbon and hydrogen. In proportion as it loſes leſs, the ſecretion of the fat will be more abundant: this then will afford an explanation of the enormous quantity of fat found in amphibious animals, fiſhes, &c. in which reſpiration is not ſo perfect, becauſe, being ſurrounded by water, they are not in contact with ſo conſiderable a quantity of oxygen as animals that breathe in the open air. They retain then more hydrogen and carbon, which paſſing into the arteries, occaſion that conſiderable ſecretion of fat, and probably produce that voluminous ſize of liver, found in fiſhes in which circulation is ſuch that almoſt all the blood goes into the liver, either to operate there a ſecretion of fat or of bile, the conſtituent parts of which do not differ much from thoſe of the former.

Children do not reſpire in the uterus, as the foramen ovale is not obliterated till towards the third or fourth year of their life, and ſometimes later: they have for the ſame reaſon a very full habit of body. What appears to confirm this opinion are the obſervations quoted in the Medical Tranſactions of London, and thoſe of Profeſſor Sandifort of Leyden, of individuals remarkable for their corpulency; in whom it was found, by diſſection, that the foramen ovale was not cloſed up; that is to ſay, that the greater part of the venous blood paſſed directly from the right into the left ventricle, to be tranſmitted into the aorta, and from thence into the whole ſyſtem, without having undergone there any of thoſe changes which are effected on the blood by reſpiration *. The lungs in ſuch individuals were diminiſhed in ſize, and in a ſtate of collapſion, which indicated that they had not diſcharged all the functions of reſpiration. This theory ſeems to throw much light on the enormous ſize of the liver

* To theſe cauſes may be added the frequent ſleep of children, oftimes in an atmoſphere of diminiſhed purity. EDIT.

in

in a fœtus, and which no one hitherto has been able to explain.

Diſeaſes of the liver, ſuch as ſchirrous tumours, &c. generally attack people who are more or leſs corpulent. The blood of perſons very plethoric, or very fat, is commonly of a darker colour and more viſcous than that of lean people, which is generally very florid. The means of cure afford a very ſtrong argument in favour of this hypotheſis; for, though obeſity be rarely the ſubject of medical treatment, yet ſome phyſicians have treated it like the ſcurvy, with vegetable acids, pulſe, &c. as affording more oxygen. It has been remarked, that people who habitually drink cyder are more meagre than thoſe who uſe beer, porter, or other liquors of the ſame kind.

There are ſome countries, the inhabitants of which are in general corpulent, as is the caſe in Holland, and ſome parts of England; while in others, as France, for example, the greater part of the inhabitants are thin. Does this ariſe from the difference of food, or is it not rather owing to the greater or leſs purity of the air? that is to ſay, to a greater quantity of oxygen contained in the atmoſphere? The above facts appear to explain the different cauſes of obeſity, and to throw great light on the means of curing various diſeaſes, which have hitherto baffled all the efforts of medicine.

III. *On the colouring Matter of vegetable Juices and the Action exerciſed on it by metallic Subſtances and their Oxyds, with a new Proceſs for obtaining Lakes of more intenſe and ſolid Colours.* *By Cit.* GUYTON. *Read in the* National Inſtitute 15 Vendem. An. 6.

LINNÆUS, the great naturaliſt of the north, ſaid, that the red colour of vegetables announced in them the preſence of an acid. It had been long obſerved, that the juice of the flower

flower of the violet acquired in tin veſſels a beautiful blue ſhade, and the uſe of it was recommended in pharmacy for the preparation of ſyrup of violets. The original colour of ſyrup which had been changed was upon this principle reſtored by long digeſtion in tin; but few philoſophers paid any attention to enquire into the cauſe of theſe phenomena, and my colleague Berthollet mentioned only as a probability in his Elements of Dyeing, that this effect reſulted from a combination of the acid with the oxyd formed at the ſurface of the tin. Such was the ſtate of our knowledge on this ſubject, when, ſtruck with the difference in the colour of two preparations of the ſame fruit, I undertook a continued examination of the circumſtances in which theſe changes took place.

I ſhall omit giving a detailed account of the comparative experiments to which I ſucceſſively ſubjected almoſt all the acid coloured fruits, ſuch as the cherry, the gooſeberry, the plum, as well as the petals of flowers, turnſol, braſil wood, turmeric, &c. by treating them in veſſels of glaſs, porcelain, metal, and metallic mixtures, or by holding them in digeſtion, either on plates of metal perfectly clean, or on metallic oxyds. I ſhall confine myſelf at preſent to reſults which may ſerve to throw ſome light on the theory of vegetable colours, or to furniſh the means of applying them with advantage to proceſſes in the arts.

My experiments proved that the red colour of fruits evidently ariſes from the re-action of their peculiar acid on their colouring matter. That tin, in reviving or reſtoring the colour of violets, only reſumes by its greater affinity the acid which made it turn red. That it is not only tin or its oxyd, as has hitherto been ſuppoſed, which exerciſes this affinity; but that lead, biſmuth, antimony and zinc produce the ſame effect; that it is ſtill more rapid and complete with iron; and that the contact of all theſe metals inclines to violet, very ſenſibly, infuſions which otherwiſe would be of a bright and ſtrong red. That the green and acid part of fruits does

not

not hold the colouring principle, disposed to become red with acids; and that the coloured part conceals the portion of the acid necessary for maintaining the state of re-action, which determines its shade. That if this colouring principle is modified in some vegetables so as to resist more or less acid or alkaline re-agents, as brasil wood in regard to acids, and turnsol in regard to alkalis, it may be brought back to that condition; which seems to indicate that, if not essentially, it is at least originally of the same nature. That all metallic oxyds are not equally proper to seize and fix vegetable colours; that some of them seem to attack the latter with the greatest ease, while others retain them only in a very weak manner. In the last place, that the new metal called tungsten carried to the utmost degree of oxydation, and which has not yet been tried in that respect, has a decided advantage above all the other metallic oxyds; that it is capable of forming lakes valuable for painting, which stand, without alteration, tests by lime water, the acetic acid or radical vinegar, sulphurised hydrogen gas, and even to a certain degree oxygenated muriatic acid gas, that enemy of colours, which suddenly burns them, and which, according to the expression of Berthollet, performs in a few moments what is effected in a long time by the combined action of air and light. This assertion will excite less surprise, when it is known that this is the only one of all the metallic oxyds which withstands the solvent power of the three mineral acids.

The oxyd of tungsten charges itself easily with the colours of all vegetables. Hitherto I have found only one exception in the petals of the *nic ago*, from which I could not extract the beautiful purple red, though I was unable to discover the cause of my failure.

In general the lakes formed with this oxyd become darker instead of lighter when diluted, and it is therefore necessary to soften the shade. I have remarked that they acquired also more intensity, when I made the oxyd assume a blue colour, by boiling it in vinegar. C. von Mons, one of my colleagues,

colleagues, being informed of the object of my researches, called my attention to the aloe. C. Desfontaines was so kind as to procure me several species, but I shall speak only of that known under the name of the succotrine aloe. I accordingly tried experiments on this plant, one of the richest in colour, though it has not the slightest appearance of it while the equilibrium of its principles is maintained by vegetable life. The woody fibre which is on the outside serves as a covering to a very viscous matter of a whitish green colour, weakly acid; but scarcely has this matter been exposed to the air when it assumes a very lively purple red colour, which becomes extremely abundant by the progress of fermentation. Of this matter I have formed lakes with alumine, the oxyd of tin, and the white oxyd of zinc: but none could bear a comparison with that prepared by means of tungsten. I have no doubt that the oxyd of this new metal may be employed in the composition of colours for dyeing, at least for the dyeing of silks which are not intended to be subjected to boiled leys. The oxyd of tin may be employed for that purpose with advantage, as it is not easily injured by acids: but our oxyd is absolutely insoluble.

I shall conclude with a reflection which may add some interest to these researches:—Wolfram, from which this oxyd is obtained, is found in the territories of France*, several mines of it are already known, and the French chemists have been employed for some years in endeavouring to simplify the processes for freeing it from foreign substances: we hope then, that the properties I have explained, will supply artists with new means to ensure duration to the productions of their genius, and open a new branch of national industry.

* It is found in abundance in the tin mines in Cornwall. EDIT.

IV. *Experiments and Obſervations tending to ſhow the Compoſition and Properties of Urinary Concretions.* By GEORGE PEARSON, *M.D. F.R.S. Read before the Royal Society, December* 14, 1797. *From the* Philoſophical Tranſactions.

[Concluded from the laſt Number, page 54.]

I SHALL next relate ſome experiments, made in order to obtain the acid ſublimate of Scheele, or lithic acid of the new ſyſtem of chemiſtry.

100 grains of an urinary concretion, which had been previouſly found to contain principally the above animal oxide, were introduced into a tube $\frac{1}{4}$ of an inch wide; which was ſealed at one end by fuſion, and which alſo was fitly bent for collecting ſublimate, and obtaining gaz. The ſealed end was coated and expoſed to fire, firſt to a low temperature, and gradually to a very elevated one.

1. Gaz was diſcharged, which had the ſmell of burning bone.

2. Water appeared boiling immediately over the charge, which ſeemed to be burning, and was turned black.

3. Gaz was diſcharged, of the ſmell of empyreumatic *liquor cornu cervi*, and about half a drachm of this liquor was in the upper part of the tube.

4. A brown ſublimate of carbonate of ammoniac appeared in the cold part of the tube; but in the hotter part, near the charge, was tar-like matter, and the gaz diſcharged had a very offenſive ſmell of empyreumatic animal oil, with which was mixed that of pruſſic acid.

The coated part of the tube was kept red hot, for ſome time after gaz ceaſed to come over.

The quantity of gaz amounted to 24 ounces, by meaſure: it conſiſted of nearly 16 ounces of carbonic acid gaz, and the reſt was air, with a larger proportion of nitrogen gaz than is contained in atmoſpheric air.

5. There

5. There was a reſidue of 30 grains, almoſt pure carbon; and 10 grains of heavy black and brown matter, a little above the coated part of the tube. In this laſt-mentioned matter were many ſmall white *ſpicula*. At about half an inch above the carbonaceous reſidue, dark gray matter had been raiſed, which weighed 15 grains.

This ſublimed gray matter did not contain any ammoniac, nor throw down any pruſſiate of iron, with ſulphate of iron. It reddened turnſole paper and tincture. It diſſolved in cauſtic ſoda; from which ſolution muriatic acid precipitated nothing; for, although on dropping it into the ſolution milkineſs appeared, the liquid ſoon grew clear again.

Ten grains of this ſublimate diſſolved in four ounces of boiling water; which being evaporated to half an ounce, there was, on cooling, a copious depoſit of white *ſpicula**. The ſublimate had a ſharp, but not ſour taſte. Being boiled in muriatic acid, and alſo in nitric, it did not diſſolve at all; but remained, on evaporation to dryneſs, in the ſame ſtate as before; and it muſt be particularly obſerved, that it left no red or pink matter, on evaporating the nitric acid from it. Sulphuric acid did not act upon it in the cold; but, when heated, it diſſolved it, without effervеſcence, from which ſolution nothing was precipitated by cauſtic ſoda: on evaporating it to dryneſs, black fumes aroſe, leaving behind only a black ſtain. This ſublimed matter did not render lime water turbid. Boiled in muriatic acid, ſo as to carry off all but a very little free acid, on the addition of lime water there was no turbid appearance, but milkineſs enſued on adding oxalic acid.

The *ſpicula*, in the 10 grains of ſublimate above mentioned, ſeemed to be of the ſame nature as the matter juſt deſcribed.

The whole of this ſublimate amounted, by eſtimation, to 18 grains; and I apprehend it is the acid ſublimate of Scheele.

* From the depoſition of theſe *ſpicula* by cooling, and from many of the following properties, they appear to be analogous to benzoic acid.

The ſublimate of carbonate of ammoniac amounted to 20 grains; and it was black empyreumatic animal oil which ſtained the tube.

This experiment was repeated, on 120 grains of a nut-brown, very light, urinary concretion. The reſult was not very different from that of the former experiment, except that the gaz contained a portion of hydrogen gaz. There were 30 grains of the above deſcribed *ſpicula*, principally mixed with carbonaceous matter: they were light, and had only a very ſlight ſharp and bitter taſte.

The experiment repeated a third time, with 80 grains of urinary concretion, afforded 15 grains of the white *ſpicula* above deſcribed, mixed with carbonaceous matter. Theſe I found did diſſolve in a large proportion of muriatic acid; which ſolution yielded them, on evaporation, in the ſame ſtate as before. Under the flame applied by the blowpipe, they firſt melted, and then evaporated, without any ſmell; leaving a ſlight black mark. Turnſole was reddened by theſe *ſpicula*.

In a fourth experiment, I found the white *ſpicula* contained in the carbonaceous matter united, on boiling, with carbonate of ſoda, as well as with cauſtic ſoda; but, as before, muriatic acid precipitated nothing from the ſolution. Theſe *ſpicula* could not be diſſolved in nitric acid; nor did the ſolution of them in water become turbid with oxalic acid. Their taſte was, as before, rather bitter and ſharp than ſour. A very ſuffocating ſmell iſſued forth, on breaking the tube uſed in this experiment, but it was not from ſulphur, nor from pruſſic acid.

Theſe experiments afford evidence of the wide difference between the animal oxide above deſcribed and the acid ſublimate of Scheele*.

* From theſe experiments, it now appears very doubtful whether the *lithic acid* of Scheele exiſts as a conſtituent of urinary concretions, or is compounded, in conſequence of a new arrangement taking place, of the elementary matters of the concretion, by the agency of fire; but it is demonſtrated, that the urinary animal oxide is really a conſtituent part, and even a principal one, of almoſt all human urinary calculi.

If

If this concluſion be allowed to be juſt, it will be neceſſary to give a name to this urinary animal oxide. Agreeably to the principles of the new chemical nomenclature, the name ſhould be *lithic oxide*. But the term *lithic* is a groſs ſoleciſm; and I truſt that philological critics will find the name *ouric* or *uric oxide* perfectly appropriate; for, if it be thought objectionable, on account of the exiſtence of the matter in arthritic as well as urinary concretions, ſtill philology will allow its admiſſion, as in other ſimilar cauſes, κατ' εξοχην; it being found in greater abundance, by far, in the urinary paſſages than in other ſituations, and therefore falling under common obſervation, as an ingredient of the urine. If, however, the term lithic oxide, or any other denomination, ſhall obtain acceptance, I ſhall very willingly adopt it.

It requires no ſagacity, in a perſon acquainted with the facts of the preceding experiments, to perceive that they are applicable to a variety of uſes in chemical inveſtigation, and in the practice of phyſic. The latter I of courſe take no notice of in this place; but, relative to the former uſes, I ſhall particularly point out, that we are now able not only to detect, in the eaſieſt manner, the *preſence* of the minuteſt proportion of the above animal oxide in urinary concretions, and alſo in other ſubſtances, but even to determine its *proportion* to the other conſtituent parts, in the ſpace of a few minutes, in moſt caſes, and in all in a very little time, without any other apparatus than nitric acid, a round-bottomed matraſs or glaſs diſh, and a lamp. By this method, I have, in a general way, examined above 300 ſpecimens of concretions, of the human ſubject and other animals, principally urinary ones; and alſo many from other parts, particularly thoſe from the joints. For theſe opportunities, I am beholden to ſeveral profeſſional gentlemen; whoſe willingneſs to furniſh me with ſpecimens, I ſhall have much ſatisfaction in acknowledging on a future occaſion. At preſent, I muſt acknowledge my obligations to Mr. Heaviſide, in whoſe muſeum I found between 700 and 800 ſpecimens. The liberal poſſeſſor of this

treaſure offered me, what I could not have taken the liberty of requeſting, namely, permiſſion to break off pieces from any of the articles, for experiment. Mr. Edward Howard did me the honour to take upon himſelf the taſk of writing down the reports, and otherwiſe aſſiſted me.

At this time I ſhall only mention,

1. That out of 200 ſpecimens of urinary calculi, not more than ſix did not contain the animal oxide above deſcribed, *i. e.* about 32 out of 33 contained it.

2. That the proportion of this oxide was very different; varying from $\frac{1}{200}$ (excluſive of water,) to $\frac{122}{200}$; but, for the moſt part, varying between $\frac{80}{200}$ and $\frac{140}{200}$ *.

3. That the common animal mucilage of urine is frequently found in concretions, in very different proportions; but is perhaps never a principal conſtituent part of them.

4. That the above animal oxide was not found in the urinary concretions, or any other concretions, of any animal but the human kind.

5. That this animal oxide was found alſo in human arthritic calculi, but not in thoſe of the teeth, ſtomach, inteſtines, lungs, brain, &c.

P. S. I think proper to ſubjoin a few experiments, made after the preceding paper was written, which afford evidence of the truth of ſome of my concluſions, and enable us to explain ſeveral properties of animal concretions.

I. *On an Urinary Concretion from a Dog.*

This calculus may be ſaid to be a great curioſity, for it is probably the only ſpecimen in London. I owe the opportunity of examining it to Mr. H. Leigh Thomas, who met with it in the courſe of his diſſections; and therefore we have unqueſtionable authority, that the concretion was

* In ſome urinary concretions, the interior part contained this oxide, and the exterior part had none of it. On the contrary, in other urinary concretions, the exterior part contained it, and the interior part did not.

really

really from the urinary bladder of a dog. It is worthy to be noticed, that the animal appeared to be in perfect health.

This concretion is of an oval figure; is three inches and three quarters in length, and three inches in breadth; is white as chalk; its surface is rough and uneven. Being sawed through longitudinally, no nucleus was found, nor was it laminated, but near the centre it was radiated, and contained shining *spicula*. In other parts it was, for the most part, compact and uniform in its texture. It weighed nearly ten ounces and a half. Its specific gravity was found to be greater than that of human urinary concretions, in general; which I have learned by experiments is also the case with urinary and intestinal concretions of other brute animals, especially with those of the horse.

The specific gravity of the present calculus was 1,7.

That of one from the urinary bladder of the human subject, of the sort called mulberry calculus, and which consisted almost entirely of uric oxide, was 1,609.

That of another human urinary concretion, of the same composition as the former, but quite smooth, extracted by Mr. Ford, was 1,571.

1. The present calculus of the dog had no taste, nor smell, till exposed to fire.

2. Under the blowpipe it first became black, and emitted the smell of common animal matter; it next smelt strongly of empyreumatic *liquor cornu cervi*; and, after burning some time, became inodorous, and white, and readily melted, like superphosphate of lime.

3. On trituration with lye of caustic soda, there was a copious discharge of ammoniac.

4. It dissolved, on boiling in nitric acid: the solution was clear and colourless; and, on evaporation to dryness, left a residue of *white bitter matter*, which, under the blowpipe, emitted, weakly, the smell of animal matter.

5. Upon distilling a mixture of 150 grains of this concretion pulverized and two pints and a half of pure water, to

three ounces, the diſtilled liquid was found to contain nothing but a little ammoniac. The three ounces of reſiduary liquid, being filtrated and evaporated, yielded 20 grains of phoſphate of ammoniac, with a little animal matter; and the reſiduary undiſſolved matter amounted to 67 grains.

6. Theſe 67 grains, being triturated with four ounces of cauſtic ſoda lye, diſcharged very little ammoniac. On diſtilling this mixture to one ounce, a very ſmall proportion only of ammoniac was found in the diſtilled liquid. The reſiduary ounce of alkaline liquid was filtrated, and mixed with the water of elutriation of the undiſſolved matter. One half of thoſe liquids, on evaporation to dryneſs, afforded a dark brown matter, amounting to 20 grains, which conſiſted of phoſphate of lime and animal matter. To the other half of the alkaline liquids was gradually added muriatic acid, which occaſioned a depoſit, in ſmall proportion, of matter that diſſolved in nitric acid, but which, on evaporation to dryneſs, left behind only a browniſh matter, conſiſting of phoſphate of lime and animal matter.

7. The reſiduary inſoluble ſubſtance in cauſtic lye, (6.) under the blowpipe, firſt turned black, and then grew white, but could not be melted.

By diluted ſulphuric acid it was decompounded. On the addition of nitrate of mercury, to the filtrated liquid, it yielded phoſphate of mercury; and, with oxalic acid, it afforded oxalate of lime; but no ſulphate of magneſia was found remaining after theſe precipitations were produced.

Theſe experiments fully demonſtrate, that the above concretion of a dog contained none of the uric or lithic oxide above deſcribed, but that it conſiſted, principally at leaſt, of phoſphate of lime, phoſphate of ammoniac, and animal matter.

The preſent inſtance leads me to explain the reaſon of the fuſibility of calculi. This is demonſtrated by the above experiments, to depend upon the diſcharge and decompoſition of the ammoniac of the phoſphate of ammoniac, during the

burning

burning away of the animal matter: hence the reſiduary phoſphoric acid readily fuſes, and, uniting to the phoſphate of lime, compoſes ſuperphoſphate of lime, a very fuſible ſubſtance.

The phoſphate of ammoniac being diſſolved out by water, or cauſtic alkaline lye, the remaining matter is infuſible, being phoſphate of lime.

A very hard, brittle, and blackiſh inteſtinal calculus of a dog, from Mr. Wilſon, was found to be of greater ſpecific gravity than human urinary calculi, and to have the ſame compoſition as that of the dog above deſcribed.

This alſo was found to be the compoſition of a white, ſmooth, round, inteſtinal calculus of a horſe, the ſpecific gravity of which was 1,791.

The ſame compoſition was diſcovered, on examining a very hard, gray, brittle, laminated, quadrilateral concretion, ſaid to be from the urinary bladder, but which, I think, was more probably from the inteſtines, of a horſe.

II. *On a Calculus from the Urinary Bladder of a Rabbit.*

This is alſo a curioſity, being the only inſtance I have ſeen. I am likewiſe indebted to Mr. Thomas for this ſpecimen, which he very kindly ſent me, fitted up as a preparation, included in the bladder itſelf. Mr. Thomas found this concretion, on diſſecting a perfectly healthy and very fat rabbit.

This ſpecimen is ſpherical, and of the ſize of a ſmall nutmeg. It is of a dark brown colour, has a ſmooth ſurface, is hard, brittle, and heavy. When broken, it appeared to conſiſt of concentric laminæ. Its ſpecific gravity was 2.

1. Under the blowpipe it grew black, and emitted the ſmell of animal matter while burning; at laſt it ceaſed to emit any ſmell; and, urged with the intenſeſt fire, ſhowed no ſigns of fuſibility.

2. It readily diſſolved, with effervescence, like marble, in both muriatic and nitric acids, giving clear ſolutions.

3. The

3. The nitric ſolution (2.) being evaporated partly to dryneſs, and partly to the conſiſtence of extract, the dry reſiduary matter was white; and the extract-like matter, which was bitter, could not be fuſed under the blowpipe; but, when brought to the ſtate of a powder, the particles of it were made to cohere looſely together into one maſs.

4. On dropping ſulphuric acid into the muriatic ſolution, (2.) turbidneſs, and a copious white precipitation, immediately enſued, from the compoſition of ſulphate of lime.

From theſe experiments it is warrantable to conclude, that the above urinary calculus of a rabbit conſiſted principally of carbonate of lime and common animal matter, with, perhaps, a very ſmall proportion of phoſphoric acid: it certainly contained no uric oxide.

I examined, in the ſame manner, a concretion which was ſaid to be from the ſtomach of a monkey; but I have not evidence of its origin equally ſatisfactory as that of the two laſt calculi. Its compoſition was found to be ſimilar to that of the calculus of the rabbit, *viz.* carbonate of lime and animal matter. Its obvious properties were alſo the ſame; it was of the ſize of the largeſt nutmeg.

III. *On Urinary Concretions of the Horſe.*

I examined ſeveral ſpecimens in cabinets, ſaid to be veſical calculi of the horſe, and found none of them to contain the uric oxide above deſcribed; but that they conſiſted (as well as the calculi from the ſtomach and inteſtines of the ſame animal) of phoſphate of lime, phoſphate of ammoniac, and common animal matter, which melted like ſuperphoſphate of lime, after burning away the animal matter and ammoniac. As theſe, and ſome other experiments, ſeemed to concur in eſtabliſhing an important truth, I thought it neceſſary to examine an urinary concretion of a horſe, which, from its figure and ſize, was unqueſtionably from the kidney of that animal; for I have found by experience, that one cannot depend entirely on the accounts in cabinets, nor indeed,

Indeed, ſometimes, on the aſſertions of perſons who collect ſpecimens.

1. This concretion, which Dr. Baillie was ſo good as to give me, was of a blackiſh colour, was very brittle and hard, and had no ſmell or taſte. It felt heavier than human urinary calculi.

2. Under the blowpipe it became quite black, and emitted the ſmell, weakly, of common animal matter. It was reduced very little in quantity, and ſhowed no appearances of fuſibility, after being expoſed for a conſiderable time to the moſt intenſe fire of the blowpipe.

3. Muriatic acid diſſolved this concretion, with effervescence, yielding a clear ſolution; which, on evaporation to dryneſs, left a black and bitter reſidue.

4. A little of the reſidue (3.) being boiled in pure water, to the filtrated liquor ſuperoxalate of potaſh was added; which occaſioned a very turbid appearance, and copious white precipitation.

5. Nitric acid alſo readily diſſolved this concretion, with effervescence. The ſolution being evaporated, partly to dryneſs, and partly to the conſiſtence of an extract, the dry reſiduary matter was white and bitteriſh, and the extract-like part ſhowed no ſigns of fuſibility under the intenſeſt fire of the blowpipe.

6. A little of the concretion, being triturated with lye of cauſtic ſoda, emitted no ſmell of ammoniac.

From theſe experiments it appears, that this calculus, like the former one from a rabbit, conſiſts of carbonate of lime and common animal matter.

A renal calculus of a horſe, in Mr. Heaviſide's collection, appeared, on examination, to conſiſt of carbonate of lime and common animal matter.

Another ſpecimen, however, of renal calculus of a horſe, in the ſame collection, marked No. 3. was found to conſiſt of phoſphate of lime, phoſphate of ammoniac, and common animal matter. It was fuſed under the blowpipe.

The

The ſpecimen marked No. 8. in the ſame collection, which was ſaid to be a veſical calculus of a horſe, appeared to conſiſt of the three ingredients juſt mentioned.

I have met with two inſtances of a depoſit of a prodigious quantity of matter in the urinary bladder of horſes, which had not cryſtallized, or even concreted: it amounted, in one ſpecimen, which was given to me by Dr. Marſhall, to ſeveral pounds weight; and in the other, which is in the poſſeſſion of Mr. Home, to about 45 pounds. Its compoſition was, principally, carbonate of lime and common animal matter *.

I have not found any inſtance of human urinary calculi of a ſimilar compoſition to that of the rabbit, and thoſe of horſes above deſcribed, which conſiſt of carbonate of lime and animal matter; and I believe that human urinary calculi very rarely occur of a ſimilar compoſition to thoſe of the dog and horſes above mentioned, which were found to conſiſt of phoſphate of ammoniac, phoſphate of lime, and animal matter, without containing *uric oxide*.

The difference in the conſtitution of urinary concretions may depend on the difference of the urinary organs of different animals, on the food and drink †, and on the various diſeaſed and healthy ſtates of the urinary organs.

I have not found the uric oxide in the urinary concretions of any phytivorous animal; but, whether it would be formed in the human animal when nouriſhed merely by vegetable matter, muſt be determined by future obſervations. In the mean time, it is warrantable to conclude, from analogy, that it would not, and the application of this fact to practice

* Since this paper was read, Mr. Blizard has been ſo attentive as to ſend me another ſpecimen of the ſame kind of depoſit as thoſe here mentioned. It now appears probable, that ſuch depoſits frequently take place, although I believe they have not been noticed before.

† I found the ſtomach-concretion called *oriental bezoar*, to conſiſt merely of vegetable matter; as did the inteſtinal concretion of a ſheep.

is obvious: but I now purpoſely avoid making any practical inferences, until I can, at the ſame time, ſtate a number of facts I have collected, relative both to concretions and to the urine itſelf.

V. *Short Account of the laſt Ruſſian Expedition for making Diſcoveries in the North-eaſt Sea. By Profeſſor* BLUMENBACH *at Gottingen. From* Geographiſche Ephemeriden. *Publiſhed by Major von* ZACH, *May* 1798.

AS very little is yet publicly known of the great ſix-years expedition, undertaken by the Ruſſians for making diſcoveries in the Northern Archipelago or Eaſtern Ocean, the following ſhort account of it, taken from the moſt authentic ſources, and particularly from the correſpondence of Dr. C. H. Merck, who was employed in the expedition as naturaliſt and phyſician, with the Royal Academy of Sciences at Gottingen, may afford ſatisfaction to thoſe fond of geographical reſearches.

This expedition was propoſed by Catherine II. ſo early as the month of November 1784. A plan was alſo drawn up for it; and the command conferred upon Captain Billings an Engliſhman, then in the naval ſervice of Ruſſia, who had accompanied Mr. Bayly the aſtronomer in Cook's laſt voyage round the world in 1776—80. Three captains of the ſecond rank were appointed under him, viz. Hall, Sariſchef and Bering, not the ſon, as Leſſeps ſays, but the grandſon of the celebrated Capt. Vitus Bering, who, on the 14th of December 1741, was interred on an iſland in the ſea of Kamtſchatka, named after himſelf, and where he had been ſhipwrecked.

The principal objects of this great and very expenſive expedition were, to ſupply all the deficiencies in regard to the important diſcoveries with which the geography of Aſiatic

Ruſſia

Ruſſia had been enriched, ſince the time of Peter the Great, by exploring that ſo little known north-eaſt corner of Aſia, the land of Tſchukt; to purſue farther if poſſible the north-eaſt paſſage, attempted by Cook; and, laſtly, to ſearch out more convenient poſts for the Ruſſian fur-trade on the north-weſt coaſt of America.

Captain Billings ſet out with his inſtructions from Peterſburgh in the end of the year 1785, and in July 1786 arrived at Ochotzk. Having paſſed the winter at Werchne Oſtrog, in the beginning of the ſummer 1787 he left the mouth of the Kolyma or Kovyma with two veſſels, the larger of which, called the Pallas, was commanded by himſelf; and the other, the Jeſaſchna, named after an arm of the river Kovyma, in which it was built, was commanded by Captain Sariſchef. This was only a preparatory expedition, the object of which, however, was nothing leſs than to double Cape Tſchelazka, (Cook's Cape North) and to proceed by this unheard-of route from the Frozen Ocean through Bering's Straits to Anadyr. I call the route unheard-of, as the romantic voyage of the Starſchina Coſſac Semon Deſchnew, in the year 1648, notwithſtanding the account of it which the Ruſſian hiſtoriographer Muller is ſaid to have diſcovered in 1736, among the archives at Jakutzk, is ſtill doubted by many ſceptics, who conſider a connection of the northern parts of both continents as poſſible.

Theſe adventurous navigators, however, could not proceed farther than to a certain point between Baranikamen and the mouth of the river Tſchaun; becauſe the impenetrable fields of ice which they found there, rendered it impoſſible for them to continue their voyage to the North-eaſt, and obliged them to return from Seredun-Kerymſky Oſtrog to Jakutzk, in order to paſs the winter.

In the mean time, Captains Hall and Bering were employed in preparations for the grand expedition. The former ſuperintended at Ochotzk the building of the two veſſels deſtined for that purpoſe, and the latter had the care of

tranſporting

tranſporting from Jakutzk the materials and ſtores neceſſary for fitting them out.

In the ſummer of 1789, the two ſhips were ready at Ochotzk for putting to ſea; when, unfortunately, the ſecond of them, the Dobrowa Namerine (the Good Intent), which was to be commanded by Capt. Hall, got on ſhore juſt at the mouth of the Ochochta; and as her keel was broken, it was neceſſary to ſet her on fire. On account of this misfortune, Capt. Billings, with his own veſſel the Slawa Roſſie (Ruſſian Glory), was not able to leave Ochotzk till towards the middle of September; at which time he proceeded to Awatſcha Bay, where he anchored in the month of October, having made in this paſſage a diſcovery of very great importance to nautical geography, as about 300 werſts from Ochotzk towards the Kurile iſlands, he fell in with a rock, an hundred fathoms high and a werſt in circumference, ſurrounded by leſſer rocks which were named Jonas Iſland, and on which many of the ſhips already loſt have, in all probability, been wrecked. Prodigious flocks of ſea-fowl come every morning from theſe rocks to the coaſt of Ochotzk, and return thither again in the evening to paſs the night.

After wintering at Kamtſchatka, theſe navigators explored, in the ſummer of the year 1790, the whole chain of the Aleutian iſlands, which ſeem to be of volcanic origin, and afterwards the large eaſtern iſlands explored by Cook; Onalaſchka and Kadjak; the bay of Cape St. Elias, &c. and returned to winter at Kamtſchatka. In the ſummer of 1791 they proceeded on their grand expedition, to ſearch for a northern paſſage through the Frozen Ocean; and having landed on Gore's and Clarke's Iſland, purſued their route from thence to the Continent of America.

As the fields of ice, which extend from the Eaſtern Cape of America, rendered it impoſſible for them to penetrate any farther, Captain Billings, in conjunction with Dr. Merck, accompanied by one of the pilots, the draftſman, two interpreters, and four ſeamen, undertook an expedition of diſcovery

through

through the country of Tſchukt from the Bay of St. Lawrence to the river Kolyma, which they had left four years before. This wonderful journey, which they performed in ſledges drawn by rein-deer and attended by ſome of the intrepid natives, continued from the middle of Auguſt till the end of February 1792, when they arrived at the river Angarka, which falls into the great river of Anuy, after having travelled through, and examined in regard to geography, natural hiſtory and ſtatiſtics, an extenſive tract of country very little known, the Bay of St. Laurence and the iſlands between Bering's Straits and the mouth of the Anadyr, inhabited by about four thouſand Tſchukteſe, who are ichthyophagi or feeders on fiſh, and the whole almoſt level land, deſtitute of wood, of the rein-deer Tſchukteſe from the above-mentioned ſtraits as far as the Kolyma.

In the beginning of May theſe enterpriſing travellers returned on horſeback to Jakutzk. Their veſſel, which they had left in the Bay of St. Laurence, had in the mean time proceeded to Onalaſchka, under the command of Capt. Sariſchef, and had wintered there, together with a ſmall cutter called the Tſchorne Orel (the Black Eagle), which had been built ſoon after their firſt arrival at Kamtſchatka, to ſupply the loſs of the veſſel ſtranded at Ochotzk, and on board which were Captains Hall and Bering.

Next ſpring both veſſels returned to Kamtſchatka. The Slawa Roſſie was left there in the harbour of St. Peter and St. Paul; but Captains Hall and Sariſchef, in the courſe of the ſummer, paid a viſit in the Black Eagle to the chain of the volcanic Kurile iſlands. They then proceeded to Ochotzk, from which they were followed, in the ſummer of 1793, by the reſt of the crew of the Slawa Roſſie in a tranſport commanded by Capt. Billings; and in the winter of 1794 the whole of the perſons employed in the expedition returned to Peterſburgh.

A full account of this remarkable and intereſting expedition is now preparing for publication, under the inſpection

of

of the Academy of Sciences at Peterſburgh. In the mean time, the Academical Muſeum at this place*, through the liberality of its worthy benefactor Baron von Aſch, counſellor of ſtate, has received a preſent highly intereſting to natural hiſtory and geography, conſiſting of works of art and natural curioſities from theſe remote regions of the northern part of Aſia, as well as of the north-weſt coaſt of America and the chain of iſlands lying between the two continents.

The ſpecimens of art of theſe polar inhabitants, and above all the needle-work of the women, (who, however, for the moſt part are troglodytes, and in their ſubterranean dwellings (*jurten*), muſt conſequently ſtrain their eyes by working at lamps filled with train oil,) exceed in elegance every thing I ever ſaw of ſuch kind of manufacture, not only among ſavages, but even among the civiliſed Europeans. As a proof of this aſſertion, I ſhall here remark, that they ſtood examination by a magnifying glaſs, under which the fineſt embroidery of Europe loſt much by being compared with them.

The aſſertion that, except food and drink, there is no object which engages more the attention of mankind than that of ornament, and that a turn for coquettery is one of the moſt general as well as moſt beneficent principles in human nature—an aſſertion ſtrengthened by this ſtriking obſervation, that though there are numerous tribes on the earth who go perfectly naked, even without ſo much as the covering of a fig-leaf, there are none, as far as we yet know from the information of travellers, who, notwithſtanding their nudity, do not ornament themſelves in ſome manner or other, I have found fully confirmed by various articles, the fruits of this voyage of diſcovery, which form part of the preſent tranſmitted to our muſeum by Baron von Aſch.

The variety and ſingularity of the appendages of the toilette of theſe polar inhabitants, condemned as it were to the

* Gottingen.

coldest climate in the world, and who have to struggle incessantly with frost and hunger, exceed all description. By way of example, I shall mention only one article, a first-rate ornament of the ladies of the Aleutian Islands, consisting of a pair of the long tusks of a wild boar, cut down to a smaller size, which are stuck into two holes, one on each side of the under lip, from which they project, and give the wearer an appearance similar to that of the wallrus, which is considered as a beauty almost irresistible.

VI. *New Process for obtaining Lemon Juice of a stronger Quality.* *By M.* BRUGNATELLI. *From* Annali di Chimia, *Vol. II. p.* 31.

HAVING observed in the course of several experiments that the slimy substance in lemon juice separates itself spontaneously from that acid, I endeavoured to find out some method of preventing it from spoiling during the time necessary for its separation, in order that the acid should not acquire any bad properties which might render it unpleasant to the taste, or uncertain in its chemical effects.

I expressed in the common manner the juice of perfectly ripe lemons, and strained it through a piece of linen. In half an hour I strained it again, to free it from a little slimy matter which had settled at the bottom of the vessel. I then added to the juice a certain quantity of the strongest spirit of wine, and preserved the mixture for some days in a well-corked bottle. During that time there was a considerable deposit, which to all appearance was of a slimy nature, and which I separated by filtering paper. If the fluid was too thick to pass through the filter, I diluted it again with spirit of wine. After this operation, the deposit remained on the paper, which was entirely covered with it, and I obtained in the vessel placed below, the purest acid of lemons combined with spirit of wine.

If

If it be required to obtain the acid perfectly pure, nothing is neceſſary but to ſeparate from it the ſpirit of wine, which can be beſt effected by evaporation. The acid of the lemons aſſumes, after it has been freed from the ſpirit of wine and the moiſture combined with it, a yellowiſh colour, and becomes ſo ſtrong that, by its taſte, it might be conſidered as a mineral acid.

It is not neceſſary to evaporate the ſpirit of wine in a cloſe veſſel, if the experiment is made only on a ſmall ſcale; nor is there any danger that in open veſſels any of the acid will be loſt, as it is too fixed to be volatiliſed by the ſame degree of heat at which ſpirit of wine evaporates. This acid has peculiar properties, which deſerve farther examination.

VII. *An Experiment to prove the Permeability of Glaſs to the Electric Fluid. By Mr.* W. Wood, *F.L.S. Communicated by the Author.*

WHETHER glaſs is or is not permeable to the electric fluid has been long diſputed among philoſophers, and various have been the experiments made both to prove and to refute the Franklinean hypotheſis.

A paper by Mr. Wilſon was read at the Royal Society, December 6, 1759, in favour of the permeability of glaſs, in which the following experiments were related: He took a very large pane of glaſs, a little warmed; and holding it upright by one edge, while the oppoſite edge reſted upon wax, he rubbed the middle part of the ſurface with his finger, and found both ſides electrified plus.

He then held the ſame pane of glaſs within two feet of the prime conductor, which was electrified plus; that part of the glaſs which was oppoſite to the conductor became electrified minus on both ſides; but, in a few minutes, the minus electricity diſappeared, and the plus continuing, diffuſed

itſelf into the place of the other; ſo that now the whole was electrified plus.

Many other experiments of a ſimilar nature were made by Mr. Wilſon, none of which, however, appear ſufficiently concluſive: I ſhall therefore ſubmit the following experiment to electricians; and if it does not prove the permeability of glaſs to the electric fluid, they will oblige me by explaining the way by which the balls in the electrometer are made to diverge without paſſing the electricity through the glaſs.

EXPERIMENT.

Cavallo's atmoſpherical electrometer was placed upon a glaſs pedeſtal, and covered by a thick glaſs receiver, ſo large as to admit of a ſpace of above two inches between its ſides and the electrometer. A charged jar was then brought within a little of the apparatus, and the balls immediately diverged conſiderably, as repreſented in Fig. 2. Plate IV. Upon touching the receiver with the knob of the jar, the diſtance between the balls was doubled; but, on the removal of the jar, they directly came together. This experiment was repeated upon the air pump, and the effects were exactly the ſame.

VIII. *Deſcription of the Apparatus employed by the Society for Philoſophical Experiments and Converſations, for producing Water by the Combuſtion of Hydrogen Gas in Oxygen Gas: with an Account of the Proceſs. Extracted from the* Minutes of the Society. 1795.

A B (Fig. 1, Plate IV.) is a light globular veſſel of flint glaſs about twelve inches in diameter, in the manner of an adopter, having the narrow necks A and B oppoſite to each other. The lower part of this veſſel is drawn out at C to form the tube C D, which is provided with a ſtop cock at its

its lower extremity D. The ſquare mahogany frame F G ſtands parallel with the horizon.

Near the end K of a mahogany board the braſs rod M N is ſcrewed ſo as to ſtand upright; it is provided with a ſhoulder, in order that it may be firmly fixed into the plate M O, faſtened at each angle to the board with ſcrews.

In the ſame manner the rod P Q is faſtened at the other end of the board K, which faſtening cannot be ſeen in this view of the apparatus. Theſe rods paſs through the ſquare frame F G at H and I, ſo that it may be ſlidden upwards or downwards on the rods.

At each hole in this frame through which the rod paſſes, a braſs ſocket (H a) is ſcrewed to the lower ſide of the frame, ſo that the rod may paſs freely through it; and that it may be fixed at any elevation, the ſocket is provided with a ſcrew H, by which the rod may be preſſed againſt the oppoſite ſide of the ſocket, and thus kept in the poſition required.

The oppoſite ſide of the frame near I is in the ſame manner provided with a ſocket and ſcrew.

The veſſel Q containing water has a tube fixed in the centre of its bottom, which receives the rod and ſlides on it, ſo that the veſſel may be ſuſtained at any required height. By means of a ſtop cock b the water may be made to trickle more or leſs quickly from the veſſel Q upon a piece of muſlin c, by which it is diſtributed over the whole ſurface of the adopter A B, and deſcends by C D into the ſquare trough D, after having ſerved to cool the adopter. d is a glaſs funnel cemented into a braſs cap at m, from which the tranſverſe tube B e, provided with a ſtop cock, may deliver the hydrogen gas of the funnel d into the adopter through the ſlender extremity e, made of iron leſt it ſhould be melted by the flame of the hydrogen gas.

Under the other neck of the adopter ſtands the funnel g furniſhed with a ſtop cock (at f) intended to receive and deliver oxygen gas by the courſe g *g* A into the adopter, in order to maintain the combuſtion of the hydrogen gas,

Through a collar of leather, fixed in the tube between i and A, an iron wire paſſes, bent in the direction A e: the end of this wire at e is at the diſtance of one-eighth of an inch from the end of the tube which ſupplies the hydrogen gas, when the wire ſtands in this poſition to deliver the electrical ſpark, and inflame the hydrogen gas as it iſſues. The end of the wire at i is ſcrewed into a ſmall braſs ball, by which it receives ſparks from the larger ball k of the electrical conductor.

The funnel containing oxygen gas enters water to the depth of three or four inches in the veſſel R S, the ſize of which admits a pint bottle of gas to be introduced under the funnel and delivered into it. The veſſel T U ſerves in like manner for the ſupply of hydrogen gas to the funnel d.

When it is intended to accelerate the combuſtion of the gaſſes, the hydrogen funnel muſt be kept conſtantly full, ſo that it may be preſſed upwards by a column of three or four inches of water: at the ſame time the oxygen funnel muſt be ſupplied ſo ſlowly that the water may riſe in it five or ſix inches above the common level. In the contrary circumſtances the combuſtion may be retarded at pleaſure; but care muſt be taken that the hydrogen gas iſſue in a continued ſtream, and the flame be maintained.

As it is neceſſary that the adopter ſhould be firmly ſcrewed to the ſquare frame, and yet eaſily ſeparable from it, the following proviſion is made:

a b is the neck of the adopter through which the gas is to be introduced. The lip at *b* ſtrengthens the mouth of the adopter. *d* is the external projecting part of the glaſs ſtopper, which is accurately ground to fit. This ſtopper being ground to a ſmaller diameter between *d* and *g* is there cemented into a braſs cap *g f*. The ſlender wire which is to deliver the electric ſpark is continued through the glaſs ſtopper *d* to i. The oxygen funnel g communicates by a narrow paſſage with the cavity of the adopter round the wire i e. The neck of the adopter is imbedded in the mahogany frame;

frame; the braſs clip *h* ſhuts upon this neck, and preſſes it to the frame by the ſcrews *i i*. To keep the ſtopper *d* firm in the neck of the adopter, a ſemicircular clip embraces the braſs cap *g f*, and meets the ſhoulder of the ſtopper *d*. From this clip two braſs pins enter the piece *k*, which being drawn to the frame by the ſcrews *l l*, forces the clip againſt the glaſs ſtopper *d*, and thus faſtens it firmly in the neck of the adopter. By the like mechaniſm the other neck of the adopter is made faſt to the frame.

The wire i e being moveable in a well-greaſed collar of leather, the braſs ball i is turned round, when the hydrogen gas has been inflamed; and the wire A e turning with this ball, the end e is removed to one ſide from the flame of the hydrogen gas; the ſcrew *m* ſerves to keep the wire in the poſition required.

Previous to the uſe of this inſtrument the funnels are to be ſcrewed off: the oxygen funnel at f; the hydrogen funnel at the neck B of the adopter. The hydrogen ſtopper being removed, the adopter is to be rinſed with diſtilled water, and, being again put in its poſition on the frame F G, is to be left to drain, the tube C D being left open. After this the adopter with its ſtoppers and included air is to be weighed: the hydrogen ſtopper being then removed, the adopter is to be filled with diſtilled water of a known temperature, and the ſtopper to be again replaced. By the weight of the water in the adopter its capacity in cubic inches is aſcertained.

The ſame ſtopper is now to be taken out and dried, and oxygen gas, under a preſſure of a two-inch column of water, to be introduced till all the water is excluded from the adopter: its orifice, being ſtill under the water, is then covered with the finger, and another perſon ſtands ready to introduce the ſtopper, which has to expel its bulk of gas, ſo that no air can enter againſt this current of the gas. The adopter is now faſtened to its frame, and the funnels are ſcrewed on. To

prevent the hydrogen tube from introducing the atmoſpheric air which it otherwiſe would hold, oxygen gas is ſucked through it from the ſmall extremity.

The funnels, the capacities of which are determined by meaſurement, and marked at the different heights, now contain atmoſpheric air confined by water, which is to be ſucked out by means of a ſlender ſyphon: when the oxygen funnel is thus emptied of its air, in order that none may remain in the neck, oxygen gas is to be introduced and ſucked out repeatedly. Then it is to be charged with the ſame gas, and the ſtop cock to be opened to allow a free communication between the gas in the funnel and that in the adopter, which is now left to drain for 24 hours, at the end of which time the water which has gathered in the tube C D is to be paſſed off by opening the cock D, which muſt juſt touch the ſurface of the water in D, that the oxygen gas in the adopter and funnel may accommodate itſelf to the preſent temperature and preſſure of the external air, which is to be noted, and then the cock to be ſtopped. The water on the outſide of this funnel is to be kept two or three inches lower than in the inſide.

The hydrogen funnel d is to be freed of its contained atmoſpheric air by the ſame means employed for freeing the other funnel, and hydrogen gas is to be repeatedly introduced and ſucked out. At laſt it is to be filled with meaſured quantities of this gas to the lip, which is to be compreſſed by a column of about four inches of water.

The apparatus is now ready. The ball k of the electrical conductor, charged by a good machine, is to be brought near to the ball i; and while the ſparks paſs in quick ſucceſſion from the wire A e to the point of the tube B e, the cock at f is to be opened quickly, ſo that the firſt portion of hydrogen gas iſſuing at e may be inſtantly inflamed; the wire is then to be turned away from the flame. The combuſtion may be accelerated by increaſing the column of water which preſſes the hydrogen gas, and leſſening the preſſure on the oxy-

gen

gen gas. The adopter is now to be kept cool by water, allowed to trickle down over it from the veſſel Q.

During the combuſtion meaſured quantities of the gaſes are to be introduced into the proper funnels; and when it is wiſhed to interrupt the proceſs, the cock which admits the hydrogen gas is to be quickly ſtopped. As the veſſels cool, the oxygen gas and aqueous vapour in the adopter will contract in bulk, and the water in the oxygen funnel will riſe towards the braſs cap f. At this moment the cock muſt be ſhut to prevent the water from riſing higher.

When the proceſs is to be renewed, the oxygen funnel is to be charged with gas, and the cock to be opened. The hydrogen funnel is next to be charged; the wire A e to be turned to its firſt poſition, and the electric ſpark to be applied as before. Thus the combuſtion may be carried on from day to day. That the electric ſparks may ſtrike quickly and vigorouſly, a communication ſhould be made with a wire between the cuſhion of the machine and the braſs cap m.

As the gaſes employed are not to be conſidered as free from azotic gas, its preſence will at laſt reduce the gas in the adopter to the ſtandard of atmoſpheric air. The flame will then become weaker, and muſt be watched, that the cock of the hydrogen funnel may be ſtopped before the flame is extinguiſhed; otherwiſe ſome of the hydrogen gas will paſs unaltered into the adopter, and be confounded with the azotic gas, from which it is not eaſily ſeparable.

When the proceſs is terminated, the quantity of hydrogen gas remaining in its funnel is to be noted from the gradations marked on the veſſel, and to be deducted from the ſum of the meaſures of hydrogen gas employed. This funnel is then to be ſcrewed off. The oxygen funnel is to be treated in the ſame manner, proper attention being paid to the level of the water, and to the temperature and preſſure of the air at the time. The adopter and ſtoppers, being now in the ſtate in which they were firſt weighed, are now to be weighed, with the contained water, to determine its weight.

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To weigh the water thus formed ſtill more accurately, and to examine its quality, it muſt be drawn off from the adopter. For this purpoſe the adopter, after it has ſtood to drain for 24 hours, is to be warmed by wrapping the upper part of it in a hot cloth: a bottle of proper ſize is to receive the extremity of the tube C D, and, the ſtop cock being opened, the expanded gas in the adopter will preſs all the water into the bottle. During the paſſage of the water, the bottle is to be held at ſuch a height that the orifice of the ſtop cock may dip only $\frac{1}{8}$th of an inch in the water; and when a ſingle bubble of gas from the adopter has iſſued through the water, the ſtop cock is to be inſtantly cloſed.

The gas remaining in the adopter is now to be transferred into another veſſel, in which it may be expoſed firſt to lime water, that any carbonic acid gas contained in it may be meaſured; and afterwards to ſulphuret of lime, which will imbibe all the oxygen gas, and leave the azotic gas in a ſtate fit for menſuration.

The quantity of heterogeneous matter introduced with the gaſes during the combuſtion being thus diſcovered, a proportionate deduction is to be made from the calculated weight of the hydrogen and oxygen gas employed. The difference of weight of the azotic gas remaining, and the common air at firſt weighed with the inſtrument, may thus be eaſily determined.

The Society from whoſe intereſting minutes we make the preſent extract, carried on the combuſtion in the manner already deſcribed, for about two hours at a time on different days, till the column of water in the tube C D was $8\frac{1}{2}$ inches in length. At each of theſe times the temperature of the gaſes and the height of the barometer were carefully noted. In the manner deſcribed by Lavoiſier, the volume of each gas at 29·85 inches of the barometer, and 54·5° of the thermometer, was aſcertained; and the weight of the oxygen gas conſumed was found to be 416·5 grains, and that of the hydrogen gas 72·5 grains; the weight of both being 489 gr. = 1 oz. 11 dwt. 9 gr.

The

The water produced weighed 1 oz. 11 dwt. 7 gr.; and, contrary to all expectation, had no ſenſible acidity.

IX. *On the Principles of Iron and Steel. By Mr.* DAVID MUSHET, *of the Clyde Iron Works. Communicated by the Author.*

THE general diffuſion of coal and ironſtone, in rich and extenſive tracts throughout the iſland of Great Britain, while it has ſtamped an additional value on land, has rendered the manufacture of iron of ſuch national importance and advantage, as to weigh conſiderably in the ſcale of our commercial intereſts. Of late years it has gained in a great meaſure that juſt preponderance to which its magnitude and general utility have fully entitled it. At preſent, however, it muſt not be concluded, that the buſineſs has attained thoſe bounds which ſet limits to its improvement; or that it has attained its utmoſt conſequence as a ſource of national and individual wealth. Its future progreſs will greatly depend upon the liberal exertions of individuals, and the ſelf-denial of the legiſlature, by abſtaining from cramping in its enlightened march the genius of this native manufacture.

Impreſſed with theſe truths, the following details will be found more allied to the practice of the manufacture of iron than attached to any ſyſtematic arrangement of ſcience; where oftentimes the practical ſtudent is obliged to ſmile, bewildered at the lengthened aſſemblage of phraſes expreſſive of a ſimple ſubſtance or meaning.

It is much to be lamented that the ſcientific arrangement of the mineralogiſt was not more conſonant to the ideas of the manufacturer; and that the labours of the former were not more directed to aſſimilate to the juſt reſults of practice his pre-conceived theories.

With heartfelt pleaſure we ſhould then peruſe the hiſtory and analyſis of the many ſtrata of argillaceous ores of iron

with

with which both England and Scotland ſo plentifully abound; their richneſs in iron, and capability of being manufactured; their contiguity to coal, and the local advantages which nature may have beſtowed, by the addition of waters ſufficient for turning machinery: theſe would form intereſting and inſtructing branches of information, and ultimately would tend to the advancement of our real intereſt.

It is alſo much to be wiſhed, for the improvement of all, that the method and order of the new nomenclature of the French ſchool would pervade every branch of chemical and mineralogical ſcience; and that the celebrated Kirwan would ſo far bend as to new model his excellent ſyſtem of mineralogy on theſe principles.

The metallic ſubſtance called iron is ſuſceptible of a greater variety of modifications, and poſſeſſed of more properties ſingular and uſeful, than any other metal. Iron, properly ſo called, is malleable. All the other ſtates of the metal contain certain ſubſtances in combination with the iron, which render it fuſible, brittle, more or leſs elaſtic, &c. Pure iron is deſtitute of foreign admixture, and is therefore perfectly malleable. A variety of methods have been eſtabliſhed to produce the metal approaching to that pure and ductile ſtate: in the purſuit of this laudable attainment the following diſtinct modifications of iron have manifeſted themſelves:

1ſt, Crude, caſt, or pig iron; 2d, Steel; and, 3d, Malleable iron. Crude iron is an eager brittle metal, obtained by the ſimple fuſion of ores in contact with pitcoal charred, or the charcoal of wood, united with a certain proportion of calcareous earth as a ſolvent or flux: its component parts are iron, carbon, and oxygen. Carbon is imparted to the iron from the fuel which is uſed in ſmelting the ore; oxygen is conveyed to the iron in a twofold manner: it exiſts in the ore in a concrete ſtate, and unites to the iron previous to ſeparation; a portion is alſo communicated by

the

the great compreſſion of air uſed to excite the degree of heat neceſſary for reduction.

Theſe are the foreign ſubſtances which alter and contaminate the quality of the iron; but, as they exiſt in different proportions in various irons, it is obvious that theſe alterations of proportion will form varieties of crude iron: the following are diſtinguiſhable:

1ſt. Oxygenated crude iron, where a ſmall portion of carbone is united to the iron, and where a ſuperabundance of oxygen exiſts. The fracture of ſuch iron preſents a white ſurface deſtitute of grain, frequently diverging in ſtreaks, reſembling an early variety of cryſtallization; ſometimes it is mottled with ſpecks of a black colour, which plainly exhibit the ſcanty portion of carbonic concretions. The external ſurface of ſuch iron always cools concave, rough, and covered with oxyd. In commerce this variety of iron is known by the names of white iron, forge pigs, ballaſt iron, &c. and its preſent value is from 5l. 5s. to 5l. 15s. per ton.

2d. Carbo-oxygenated crude iron*, or iron united with equal portions of carbon and oxygen. The fracture of this iron is light grey, preſenting a ſmall diſtinct grain; the ſurface of the pig partially punctured, leſs concave, and more free from oxydation in cooling than the former ſtate. In commerce it is known by the names of grey iron, No. 3,

* I am fully aware of the novelty of this term, and that by ſome I may be liable to be cenſured. I have, however, the great Lavoiſier for my precedent, who, in naming the compound radicals of the vegetable kingdom, uſed hydro-carbonous and hydro-carbonic, to expreſs the ternary combination of hydrogen and carbon with oxygen; in the former, into that of an oxyd—in the latter, into that of an acid. It may however be remarked, that in this way the prevailing ſubſtance was marked by a precedency given to its ſign, which implied that there exiſted a larger proportion of this than the following ſubſtance. Let the preſent, however, expreſs the combination of iron with carbon and oxygen in equal portions: the precedency given to carbon in beginning the compound, is meant to denote its ſuperior agency, and its preſence being eſſential in the manufacture of crude iron.

&c.

&c. and its preſent value is reckoned from 5l. 15s. to 6l. 5s. per ton.

3d. Carbonated crude iron, wherein carbon fully predominates, and where an extra privation of oxygen has taken place. The fracture of this iron is dark grey inclining to blue, preſenting large brilliant concretions of a metallic luſtre in the centre of the pig, but diminiſhing in point of ſize as they approach the ſurface. In this quality of iron the upper ſurface, which is left to cool in contact with the open air, after being run from the blaſt furnace, is partially convex and full of punctures: it is then called by the workmen honey-combed, and its quality is often inferred from the ſize and depth of the puncture: its tendency to oxydation in cooling is ſtill leſs than that of the ſecond variety of crude iron. This quality is well known to the manufacturer and founder, under the names of No. 2, good melting pig iron, &c. Its preſent value is from 6l. 15s. to 7l. 10s. per ton.

4th. Super-carbonated crude iron, or iron ſaturated to exceſs with carbon, and having united to it but a ſcanty portion of oxygen. The fracture of this iron preſents an aſtoniſhing group of large, regular, metallic grains, each preſenting a prominent edge outwards; in this ſtate the ſaturation of carbon has been ſo complete, that it exiſts united to the iron in the ſtate of plumbago; luſtre rich and brilliant, inclining to dark blue; the upper ſurface ſmooth and convex, frequently covered with a beautiful coating of ſhining plumbago, and completely free from oxyd. This is the No. 1, and ſmooth-faced iron of the manufacturers: its preſent value is 8l. to 8l. 10s. per ton.

Theſe are the principal varieties of crude iron. A number of intermediate qualities are produced, exactly dependent upon the quantity of mixtures, and their relative proportions to each other. The greater the quantity of charcoal uſed in ſmelting the ore, the more ſaturated with carbon and the

more

more valuable will be the pig iron. A ſparing proportion of charcoal in the blaſt furnace is productive of effects completely contrary to theſe.

It moſt naturally occurs to enquire, to what extreme degree of combination theſe alterative principles are capable of with iron, and in what ſtate the metal is when this exceſſive ſaturation is completed.

If crude iron is expoſed in fuſion, for a length of time, to abſorb carbon, and at the ſame time is protected from oxydation, it will receive this principle to ſuch exceſs as to form a true plumbago. In this ſtate it either reſembles ſteel, grained lead ore, or aſſumes an imperfect cubical cryſtalliſation, wonderfully brittle, and poſſeſſed of a real metallic appearance and weight: in this ſtate it is powerfully attracted by the magnet.

Specimens of this ſingular combination are extremely rare; ſeveral that I am in poſſeſſion of, have regular faces approaching to a cubical ſtructure, with ſurfaces chequered with numerous diagonal lines interſecting each other, and forming rhombuſes: theſe are eaſily diſplaced with a knife, and exhibit them compoſed of the thinneſt laminæ imaginable: in this ſtate they are not obedient to the magnet, unleſs a conſiderable number of them are attached together. The ſpecific gravity of this mixture of iron and carbon is to water as 6·9694 to 1·000; a cubic foot weighs 435·58750 pounds avoirdupoiſe.

In manufacturing crude iron, the higheſt poſſible pitch to which it can be united with oxygen is productive of a coarſe metallic ſubſtance, whoſe fracture is dark and porous, and whoſe ſurface on cooling becomes covered with a deep earthy oxyd; incapable of being fuſed but by withſtanding a violent heat; capable of receiving ſome impreſſions of malleability, though ſtill poſſeſſing exceſſive brittleneſs when cold. The ſpecific gravity of iron thus united with oxygen is 6·5325.

Theſe ſtates of extreme ſaturation of carbon and oxygen only

only relate to theſe principles as they become united to the iron in fuſion: they may alſo be preſented to crude iron in the proceſs of cementation; in which caſe carbon unites to iron in an aëriform ſtate, diſtending the particles and ſoftening the maſs: oxygen, on the contrary, attacks the metal, and reduces it to a dark blue oxyd deſtitute of metallic brilliancy and weight; but of this more hereafter.

Steel, from its great affinity to crude iron, ought next to be mentioned; but as manufacturers hitherto have placed it as the third exiſting ſtate of the metal, it will be mentioned in that order.

To produce malleable iron in its pure ſtate, many and various have been the proceſſes adopted: theſe however have all in ſome meaſure fallen ſhort. Malleable iron ought to poſſeſs no foreign mixture whatever, to be in a ſtate of purity; but as the modes of operation have hitherto conſiſted in manufacturing this ſtate of the metal from crude iron, and as crude iron is always found to contain principles inimical to malleability, it is obvious, that the quality of malleable iron will at all times depend upon the degree of expulſion of the alterative mixtures contained in the crude iron; the deſtruction of which, and the conſequent malleabilization of the iron, conſtitute the univerſal acknowledged principles of bar-iron making.

From the imperfect diſſipation of oxygen and carbon in the proceſs of malleability, ariſe the various qualities of malleable iron; theſe may be arranged in the following order: 1. Hot ſhort iron; 2. Cold ſhort iron; and, 3. Iron partaking of none of theſe evils; and ſo far it may be denominated pure malleable iron.

1ſt. Hot ſhort iron is poſſeſſed of an extreme degree of fuſibility when in contact with a high degree of heat, and is incapable of receiving the weight of a ſmall hammer without diſſipating; it is, however, poſſeſſed of an extreme degree of ſoftneſs and ductility when cold, and may then be bent or twiſted in almoſt any direction. Various reaſons have

have been aſſigned for this deſtructive property in hot ſhort iron. I am of opinion, that it ariſes from the iron containing a ſmall portion of concrete carbon, not extirpated during the operation of rendering the iron malleable; and that in proportion to the quantity of carbon united, ſo will be the ſhortneſs or fuſibility of the iron: this variety of iron is always of a dark-coloured unmetallic fracture.

2d. Cold ſhort iron is poſſeſſed of the property of withſtanding the moſt violent degree of heat, without exhibiting the leaſt indication to fuſion; it remains firm under the heavieſt hammer, and is capable, while hot, of being beat into any ſhape: when cold, however, it is brittle, and poſſeſſed of a ſmall degree of tenacity: its fracture is always clear and large-grained, of a light blueiſh colour. A ſmall portion of iron diſſolved in the phoſphoric acid is now believed to conſtitute the cold ſhort principle of iron. Beſides the difficulty of conceiving how an acid could exiſt in the violent and long-continued heats of the refinery, the puddling and balling furnaces, wherein the metal is ſubjected to motion, frequently agitated, and extremely divided, how does it happen, that that iron on which the cold ſhort principle is impreſſed, becomes more and more cold ſhort, by a continued expoſure to the combination of oxygen with caloric, either excited by blaſt or the attenuated heat of a wind furnace? This fact would imply a generation of the alterative principle—which is indeed the caſe—but which cannot be admitted, if the cold ſhort quality is attributed to the phoſphat of iron; unleſs recourſe is had to the ſuppoſition of a new combination of this metallic ſalt during the operation.

If highly oxygenated crude iron, of any manufacture, is expoſed to the action of a current of flame, after its ſmall portion of carbon is burnt out, and after the maſs has exhibited the proper ſigns of malleability, it will paſs into the ſtate of cold ſhort iron; and this principle will exiſt in proportion to the length of the expoſure; or, in other words, in

proportion to the oxygen preſented to the metal, and its tendency to quit the caloric to unite with the iron.

3. Pure malleable iron derives its ſtrength, tenacity, malleability, and ductility, by being totally deprived of the principles which conſtitute the cold and hot ſhort qualities of iron. This is effected in the courſe of rendering it malleable, either by the attention of the workmen, or from the proper quality of the crude iron uſed: its fracture is generally clear, conſiſting of ſmall regular dark blue grains; by much hammering the iron commonly gains fibre, and is then of a light blue colour, uncommonly tenacious when cold. The excellence of pure malleable iron is alſo manifeſted by the aſtoniſhing degree of heat it withſtands without exhibiting the leaſt ſign of fuſion, or without loſing much of its metallic parts by oxydation.

A line of diſtinction ought to be drawn betwixt the iron produced with wood-charcoal and pit-coal. As the preſent relation of the ſimple principles of the metal does not immediately interfere with that diſtinction, it will more properly arrange itſelf along with the obſervations on the various modes practiſed for rendering iron malleable.

However variouſly conducted the modes of operation are at different works, and in different countries, to produce malleable iron, yet the principle of operation is the ſame, namely, that by diſſipating the carbon and oxygen, contained in the crude iron, bar or malleable iron is the reſult.

Furnaces of a multiplicity of ſhapes have been erected for this purpoſe; but in the moſt perfect conducted proceſſes hitherto, it has been found, that a heavy loſs of metallic parts accompanied the manufacture: 40, 35, to 30 cwt. of crude iron have been uſed to fabricate 1 ton of finiſhed bars; the quantity uſed always depending upon its aptneſs to become malleable, the ſkilfulneſs of the workmen, the operation adopted, and the quality of the malleable iron wiſhed to be produced. Theſe obſervations more immediately relate to the home manufactures of iron with pit-coal; but

but in many inſtances they will alſo apply to thoſe of other countries, where the charcoal of wood is uſed for fuel.

Since crude iron exiſts of ſuch a variety of quantities, owing to the various proportions of mixture united with it; and ſince it is almoſt univerſally uſed to produce bar or malleable iron; it is natural to infer, that there muſt exiſt one particular variety of it, which could be appropriated to the manufacture in preference to any other. Theory ſays that that crude iron, carbo-oxygenated, which contains the alterative principles in equal portions, requires only to be expoſed in a fluid ſtate to the action of fire, either in a wind furnace or ſmall blaſt. By this expoſure the carbon becomes volatilized, and carries off the oxygen along with it*. Practice has however confined the operation chiefly to the forge pig (oxygenated crude iron). This variety of iron becomes ſooner malleable, but is likewiſe ſuſceptible of early oxydation, and conſequently liable to become cold ſhort. Neither can it unite to bar iron thoſe properties from whence are derived great ſtrength and ductility.

When carbonated crude iron is uſed, the waſte then is apt to be exceſſive: the metal retains for too long a period its fuſible principle, which muſt neceſſarily expoſe the maſs to a longer continued action of the flame, whereby oxydation on the metal in a fluid ſtate takes place, and a conſiderable portion of it is deſtroyed before the iron exhibit ſigns of infuſibility. Malleable iron made from this ſtate of the metal has a great tendency to be red ſhort, and loſes alſo conſiderably of its weight under the forge hammer.

It has at all times been aſſerted, that crude iron contains a conſiderable proportion of its parts, by weight, inimical to malleability; and that, in the operation of refining, it then

* A definition more conſonant to chemical language would be to ſay, that the oxygen unites to the carbon and forms carbonic acid, which is expellable even in a moderate heat. The eſcape of the laſt portion of the acid is indicated by the diſappearance of fuſion, and the coaleſcence of the clotted iron.

parts with this proportion of mixture, which renders the remainder malleable. A concluſive inference from this would be, that ſome crude irons contain one half, ſome three fourths, and others again an equal portion of mixture for iron; ſeeing theſe are the proportions loſt by iron in the operation of rendering it malleable. The miſchief with which this fallacious opinion is fraught is inconceivable; eſpecially as it has been ſupported by men who have laid claim to ſcientific and practical abilities: the belief of it ſlackens the induſtry of individuals to attempt leſſening the loſs of real metal; on the contrary, workmen are taught to look upon a large proportion of it as incapable of being metallized, and as only fit for deſtruction.

If manufacturers of bar iron would more frequently deprive a given weight of the ſcoria of the refinery and puddling furnaces, of its iron, they would be more able to eſtimate the portion of unmetallic parts contained in their crude iron: upon finding the ſcoria to contain 30, 40, to 50 per cent of iron, equally fit for converting into malleable iron as any part of the original maſs, their attention would be more frequently arreſted, and employed to deviſe means, either to prevent the eſcape of ſuch a conſiderable proportion of iron, or to fuſe ſuch ſcoria ſo as to deprive it of the laſt portion of metal.

The relative proportion by weight of carbon and oxygen united to crude iron is ſmall indeed: the poſſible proportion in which they can exiſt will be more eaſily conceived by adducing the weight of a ſpecific bulk of each and their analogy.

1 Cubic foot of oxygen gas	·0792859 lb. av.	S. gravity ·0010530880
1 ditto carbon	24·519	S. gravity ·39214
1 ditto carbonated crude iron	453·70325 lb. do.	S. gravity 7·2593680

The vaſt diſproportion betwixt the cube of iron and the aggregate of the mixtures renders further illuſtration on this head ſuperfluous *, though at ſome future period I hope to be

* I wiſh not to be underſtood as if I meant to ſay that the quantity of oxygen in a concrete form in iron is to be conſidered as having no greater ſpecific

be able to ſtate the exact weight of mixtures united with a given quantity of crude iron.

Some, however, may contend for the exiſtence of ſome metallic ſubſtances along with crude iron, which may alſo form part of the unmetallic maſs: we are however unacquainted with any capable of enduring the violent heats uſed in manufacturing iron, excepting manganeſe; and this ſemi-metallic ſubſtance is found in bar iron and ſteel, nearly in the ſame proportion as in crude iron: hence it can form no part of the *ſuppoſed* heterogeneous matter expelled during the proceſs of malleability.

Steel is a mixture of iron with carbon in an aëriform ſtate *. Carbon is given to iron by heating it violently, unexpoſed to air, in contact with charcoal duſt. The proportion in which carbon exiſts in ſteel is various, depending upon the degree of purity exiſting in the malleable iron previous to cementation. In abſorbing this principle it gains weight; and this augmentation of weight, by the addition of carbon, is dependent upon the reaſon already mentioned. Some iron, in paſſing to the ſtate of ſteel, gains $\frac{1}{140}$th part of its original weight, while others gain not more than $\frac{1}{175}$th part.

ſpecific gravity than an equal bulk in a gaſeous ſtate, that is combined with caloric, inſtead of being combined with the metal. The enormous waſte of real metal, in converting a given quantity of crude into malleable iron, is generally ſo great, that, at preſent, I merely wiſh to call the attention of artiſts to the prevention of this waſte, inſtead of ſatisfying themſelves with ſaying they had only a given product, becauſe the crude iron employed was of ſuch a nature as to be incapable of yielding more.

* In the works of thoſe who have treated on iron, I have never yet ſeen carbon which exiſts in crude iron, diſtinguiſhed from that abſorbed by malleable iron, in the proceſs of converting into ſteel. I could adduce many facts, which to me appear concluſive, to prove that carbon exiſts in crude iron in a concrete ſtate, ſeparable by mechanical diviſion; and that it is united to ſteel in a gaſeous ſtate by the combuſtion of its baſe, inſeparable in any form by the moſt minute mechanical reduction.

If malleable iron was entirely freed from carbon in the manufacturing, the inference would be juſt, to ſtate that the weight gained was the exact meaſure of carbon neceſſary to form ſteel; but as ſome malleable irons afford a conſiderable portion of carbon, it is evident that the total meaſure of carbon will depend upon the pre-exiſting quantity contained in the iron. The greater the quantity introduced the more brittle will the quality of the ſteel be found, and the leſs capable of preſerving its nature and a ſolid form, when expoſed to intenſe degrees of heat.

Steel, at certain degrees of heat, poſſeſſes all the ſoftneſs and malleability of iron; but, when cold, partakes of the eager brittleneſs of crude iron. When properly hammered, it gains fibre, and then becomes the moſt elaſtic ſtate of the metal. It is alſo capable of fuſion at a high degree of heat: it may then be run liquid into iron moulds, and afterwards beat out into the moſt ſolid ſhapes, poſſeſſing coheſion and cloſeneſs of grain, with an aſtoniſhing degree of flexibility and tenacity. In this ſtate it is called caſt ſteel, and is uſed for fine inſtruments, where durability, poliſh and edge are requiſite.

In ſo far therefore as ſteel is capable of fuſion, without deſtroying its nature; and as it contains iron and carbon alone, it muſt be conſidered as a variety of crude iron free of oxygen, and ſo far is partially malleable. The fuſion of ſteel deſtroys its property of being welded. This change of nature is occaſioned by the addition of oxygen in the caſt ſteel furnace: hence we find, that fuſion is incompatible with an extreme degree of malleability.

Since a ſmall portion of oxygen is added in the fabrication of caſt ſteel, the component parts of this modification of the metal will be, iron, carbon, and a ſmall portion of oxygen. Admitting then for a moment that the carbon is in the ſame ſtate as in crude iron, here then exiſts a ſimilarity of principles with carbonated iron. Yet there are few but know

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the very wide difference betwixt crude iron and caſt ſteel when applied to uſe.

This amongſt many other poſitions might be adduced to prove that carbon exiſts united in a gaſeous ſtate to iron, and conſtitutes ſteel; or, in other words, that iron to conſtitute ſteel muſt hold in ſolution a portion of carbon, not leſs than $\frac{1}{175}$th part of its own weight.

Some of the foreign writers account for the difference betwixt ſteel and crude iron, by affirming that the former is iron *perfectly reduced* to which carbon is given, and that the latter is only iron *imperfectly reduced*, and which alſo contains carbon. This is a feeble diſtinction. Iron knows of no ſtate of imperfect exiſtence as a metal; the particles of iron in a crude ſtate are equally diſpoſed to become malleable, ſo ſoon as the carbon which has interpoſed itſelf betwixt them is burnt out. The ſame operation muſt take place before ſteel is brought back to the ſtate of malleable iron; hence, according to the reaſoning of the French writers, they are both imperfect ſtates of the metal.

Crude iron is only ſo far imperfect, becauſe it contains carbon in a material ſtate. Steel may be ſaid to be more perfect, becauſe it is united to carbon in an elaſtic ſtate; and iron, properly ſo called, is perfect in proportion as it is void of theſe impurities.

Steel alſo poſſeſſes the ſole property of acquiring a degree of hardneſs, when immerſed at certain degrees of heat into cold water, which enables it to perforate and act upon all other metallic ſubſtances. This is occaſioned by the ſudden expulſion of the caloric from the interſtices of the metal, which brings on an inſtantaneous aggregation of the particles, and which renders the fracture uncommonly cloſe and regular. Expoſure to a ſimilar degree of heat, and cooling in the open air, brings it back to its uſual ſtate, capable of being again and again hardened, and not to the ſtate of malleable iron, as a late eſſayiſt on iron has erroneouſly ſtated.

Having thus ſimply ſtated the principles, and the various modifications of iron; thoſe operations which have been practiſed for obtaining the various ſtates of the metal come next to be related; prefaced by ſome account of the materials uſed; the manner in which they are obtained; their preparation and application to uſe; previous to which, I ſhall forward, for the next number of the Philoſophical Magazine, a few remarks relative to the hiſtory of the manufactures of iron and ſteel in Britain, which may not prove unacceptable to thoſe who feel themſelves intereſted in this important branch of national induſtry.

X. *Account of the* Canis Graius Hibernicus, *or Iriſh Wolf-dog, deſcribed in Pennant's Hiſtory of Quadrupeds, third edit. Vol. I. p.* 241. *By* A. B. Lambert, *Eſq. F.R. and F.L.S. From the* Tranſactions of the Linnean Society, *Vol. III.* 1797.

THIS drawing of the Iriſh wolf-dog was given me by lord Altamont; done exactly the natural ſize of one in his lordſhip's poſſeſſion, at Weſtport in the county of Mayo, Ireland. During my ſtay there in 1790, I had frequent opportunities of obſerving theſe dogs; lord Altamont having eight of them, the only ones now in the kingdom. There is a man employed on purpoſe to take care of them, as they are with difficulty bred up and kept healthy.

I took the meaſurement of one of the largeſt, which is as follows: From the point of the noſe to the tip of the tail ſixty-one inches, tail ſeventeen and a half long; from the tip of the noſe to the back part of the ſkull ten inches; from the back part of the ſkull to the beginning of the tail thirty-three inches; from the toe to the top of the fore-ſhoulder twenty-eight inches and a half; the length of the leg ſixteen inches; from the point of the hind toes to the top of the hind

hind ſhoulders thirteen inches; from the point of the noſe to the eye four inches and a half; the ears ſix inches long; round the wideſt part of the belly (about three inches from the fore-legs) thirty-five inches; twenty-ſix inches round the hind-part cloſe to the hind-legs; the hair ſhort and ſmooth; the colour of ſome brown and white; others black and white.

They ſeem good-tempered animals, but, from the accounts I received, are degenerated in ſize. They were formerly much larger, and in their make more like a greyhound.

The following extract of a letter from the Earl of Altamont to A. B. Lambert, Eſq. V. P. L. S. read to the Society Jan. 3, 1797, contains ſome farther particulars reſpecting theſe animals.

"There were formerly in Ireland two kinds of wolf-dogs, the greyhound and the maſtiff. Till within theſe two years I was poſſeſſed of both kinds, perfectly diſtinct and eaſily known from each other. The heads were not ſo ſharp in the latter as in the former; but there ſeemed a great ſimilarity of temper and diſpoſition, both being harmleſs and indolent. The painting in your poſſeſſion is of the maſtiff wolf-dog. (See Plate V.)

"I have at preſent five wolf-dogs remaining, three males and two females; in theſe the two ſorts appeared to be mixed. The dam was of the maſtiff, the ſire, if I am not miſtaken, was of the greyhound kind. The ſire and dam had not dwindled in ſize from any that I remember here. Thoſe which now remain are too young to judge of. We have an old man here named Bryan Scahil, now in his 119th year, whoſe memory ſeems accurate and all his faculties complete: he perfectly remembers the hunting of wolves in Ireland as a common matter of ſport; and informs me, that the uſage was, to collect all the dogs of every ſort in the neighbourhood, and to borrow wolf-dogs from the principal gentlemen, who alone had them, and who uſually aſſiſted in

the chace. A tenth part of the dogs used were not wolf-dogs, which never were in any number in the hands of the common people. I conceive also, that these dogs having *no nose*, other kinds were necessary to find the game and follow the scent of it. Scahil described wolves with such perfect accuracy, that I have no doubt of his being well acquainted with the animal."

XI. *Observations on Platina, and its Utility in the Arts, together with some Remarks on the Advantages which reflecting have over achromatic Telescopes. By* Alexis Rochon, *Director of the Marine Observatory at Brest. From the* Journal de Physique, 1798.

[Concluded from page 27.]

FORGED PLATINA.

THE reader must recollect that all metals are easily melted after they have been reduced into very thin plates. Thus gold leaf melts at the flame of a taper, though not excited by a blow-pipe. In this manner also philosophers are enabled to melt those small glass globules which serve them as simple microscopes. This observation is sufficient to indicate the process that must be pursued in the operations in regard to forged platina. The platina is first purified by means of nitre and sandifer (*sel de verre*); this mineral is then divided into very minute particles, either by trituration, or by solution in nitro-muriatic acid or diluted nitric acid, according to the method of my colleague Tillet.

Nitre, antimony, arsenic, and, in short, every substance which becomes volatilised, may be employed for this operation; but arsenic ought to be preferred, because it is more easily dissipated. The platina being thus divided into exceedingly small particles, must be exposed to a strong fire, which must be excited still more by the oxygen gas that disengages

diſengages itſelf from nitre in combuſtion. In this violent fire the particles of the platina become agglutinated; and by forging them at a white heat repeated times, the workman is able to give them ſufficient adheſion to ſtand being hammered into plates. The well-known labours of Sickengen on forged and laminated platina render it unneceſſary to ſay any thing further on this ſubject. He allowed me to ſee his proceſs; and to that favour I am indebted for the knowledge neceſſary to enable me to treat a difficult ſubject abſolutely foreign to my ſtudies.

CAST PLATINA.

Caſt platina is infinitely more intereſting to the arts; and it is on this labour that I have beſtowed my chief attention. The proceſs which perfectly ſucceeded with me, to caſt crucibles and large pieces of platina of different forms, was, to melt the metal according to the method of Scheffer, by a mixture of arſenic and ſandifer. The fire muſt be moderate; and for this reaſon there muſt be at leaſt ten pounds of arſenic and four pounds of ſandifer to melt one pound of platina. The maſs then will be in a ſtate of perfect fluidity when poured into the mould, though the fire be ſcarcely ſufficient to fuſe ſilver. Platina in this ſtate is exceedingly fragile and brittle; were it expoſed to a red heat, the operation would abſolutely miſcarry. The arſenic, by diſengaging itſelf too rapidly, would reduce it to ſcales, which would no longer have any adheſion. It may readily be conceived, that this accident muſt have occurred to me more than once. I have, however, been able to avoid it by incloſing the pieces of platina which I had moulded in a box of plate-iron, filled with ſand and pounded charcoal. I then expoſed them for more than a month to a fire graduated from the heat of boiling water to that which fuſes ſilver. Platina in this ſtate no longer reſembles a metal: it might rather be taken for a metallic calx. The particles which

compoſe

compoſe it are very cloſe; but they have only a very feeble adheſion, like that of an earthen veſſel dried in the ſhade. It is then that the platina muſt be expoſed to the moſt violent fire; and when the metal has undergone that operation, it reſumes its natural ſtate, becomes ſonorous, malleable, and the ſtrongeſt heat gives it always new degrees of improvement.

A little time after I had read this memoir, the learned Dr. Ingenhouz begged me to unite into a maſs for him about two ounces of platina, which he had carefully purified by means of the nitro-muriatic acid. I was obliged to incloſe in a very thin leaf of platina all the fragments of this metal, which were too ſcattered and too minute to be ſubjected ſeparately to the action of the fire and of the ſtamper; but, when thus united, I gave them the higheſt degree of heat poſſible to be produced from charcoal excited by a pair of bellows; and I ſoon obtained, by ſtriking them with the ſtamper, a ductile and malleable maſs. After the ſucceſs of this experiment, I employed myſelf in purifying in the fire, and in a crucible, platina in grains, by means of nitre and ſandifer, which muſt be afterwards waſhed in the nitric acid; and by ſtriking at a white heat thoſe grains contained in the laminæ of platina, I procured at a ſmall expence conſiderable maſſes of malleable platina. This proceſs will render unneceſſary hereafter the uſe of the oxyd of arſenic, unleſs the workman wiſhes to obtain, by caſting, large crucibles or muffles of platina.

I was the firſt who treated platina in a large maſs in a manner truly uſeful to the arts: and though I have paid a great deal of attention to this labour, aſſiſted by the information of the learned Iſquierdo and the metallurgic talents of Daumy junior, I have employed my chief care in improving the mixture of platina with tin and the oxyd of arſenic, for the conſtruction of large ſpecula for teleſcopes, and, above all, for the ſpecula of ſextants and other inſtruments for determining

termining the longitude at ſea. In the latter labour I have been well ſeconded by Carrochez and other artiſts who worked under my direction.

Being fully ſatisfied with the trials, of which I have given an account in the preceding memoir, I was deſirous of conſtructing a teleſcope like that of Herſchel. For this purpoſe, I engaged in the year 1790 a friend of the arts and ſciences, the unfortunate Trudaine junior, to procure from Herſchel a good ſeven-feet teleſcope. The ſuperiority of the teleſcopes made by that able aſtronomer depends much more than generally believed on the Newtonian conſtruction which he has adopted. The Gregorian teleſcopes are by no means to be compared to the former. An appearance of convenience made them however to be commonly adopted, though that conſtruction preſents great difficulties to be ſurmounted in the complete execution of it. But as it is always neceſſary in the uſe of teleſcopes, the magnifying powers of which are conſiderable, to adapt a magniſier that embraces a wide field, in order to find readily in the heavens the ſtar to be obſerved, it may eaſily be perceived that it is a matter of indifference whether the teleſcope employed for that purpoſe be of the Gregorian or Newtonian conſtruction; becauſe in that caſe a direct or lateral view of the object is abſolutely indifferent. The lateral ſight is indeed much more convenient when the Newtonian teleſcope is well diſpoſed on its ſtand. Herſchel has adopted for his teleſcopes a conſtruction ſtill more ancient and ſimple; and though it approaches near to the origin of the art, it is no leſs uſeful when uſed for large inſtruments. I have admired the effects of it in his teleſcope of twenty feet; and I ſhould have been enabled to form a better judgment of it in that of forty feet, but there then ſtill remained ſome improvement to be made in the latter, which is unique in the annals of aſtronomy. It is certain that a ſmall inclination given to a concave mirror of a long focus does not ſenſibly diſfigure the image formed in its focus. This ſmall inclination is ſufficient to permit the obſerver to look through a ſtrong magniſier at the image

of

of the ſtar, without intercepting, by the head, the rays which proceed from it.

In the beginning of the year 1791, Trudaine entruſted to me the teleſcope which he had procured at London. Aſtronomy is indebted to Herſchel for the nobleſt diſcoveries of this century. The name of that obſerver will be tranſmitted to the moſt diſtant ages. He has augmented our planetary ſyſtem, and enriched it with a new planet, accompanied with ſeveral ſatellites. In 1671, Dominic Caſſini, obſerving the moon with an excellent teleſcope of Campani, remarked, in the obſcure part of her, ſome ſmall whitiſh nebulæ. This intereſting phenomenon did not eſcape the teleſcopes of Herſchel; and after the year 1784, that able aſtronomer announced, that he had ſeen a burning volcano in the middle of the ſpot called *Ariſtarchus*. We find in the Memoirs of the Academy of Sciences for the year 1706 the following remarkable paſſage of De la Hire: "The ſmall ſpot of Ariſtarchus is ſo brilliant, that ſome have believed it to be a volcano."

Are theſe ſmall whitiſh nebulæ bodies luminous of themſelves, or do they enjoy only a borrowed light? Such is the queſtion which I propoſed to reſolve, by aſſiduouſly obſerving theſe ſmall nebulæ, both with Herſchel's teleſcope, and that which I cauſed to be conſtructed of platina in the courſe of the year 1791, on the model of that of the above aſtronomer. Being deprived, ſince September 1792, of the uſe of that inſtrument*, the conſtruction of which had coſt me great

* Teleſcopes of ſtill greater magnifying powers might be ſo eaſily procured at Paris, that thoſe who are truly intereſted in the progreſs of the arts and ſciences are not a little aſtoniſhed that the great teleſcope of ſixty feet, announced in all our journals, is ſtill a project; and that no one even ſo much as thinks of realiſing it. Though the art of making large ſpecula is attended with ſome difficulties, it however requires neither the ſame talents nor the ſame patience as that of conſtructing large achromatic object glaſſes. The catoptric teleſcope requires only the regular labour of a ſingle ſurface, and it is the work of a few moments which forms or deforms the poliſh of the concave ſurface of a large ſpeculum.

labour

labour and much money, I was not able to continue this kind of obſervations.

This teleſcope of ſeven feet, which I conſider as ſuperior to that of Herſchel of the ſame length, enabled me, during the time I had the uſe of it, to be convinced that the ſpot Ariſtarchus, if not a volcano, is at leaſt a body luminous of itſelf. The grounds on which I found my opinion are agreeable to the principles explained by Bouguer in his Treatiſe on the Gradation of Light; for thoſe ſpots of the moon which reflect moſt ſtrongly the direct light of the ſun did not exhibit to me in the ſhade the ſame appearances as Ariſtarchus, whatever efforts I made to augment the intenſity of that feeble light which ariſes from the reflected rays of the earth. Beſides, it is eaſy to conceive that a very lively fire, concentrated in the hollow of a crater, cannot diſſeminate the light which emanates from it: in that particular caſe, like a reverberator, it cannot be ſeen but under certain circumſtances. It is remarked then, that it is in the months of Pluvioſe, Ventoſe and Germinal that this volcano is moſt *apparent*.

When I undertook a journey to London, by order of Government, for the benefit of the ſciences, I employed myſelf in a particular manner in the improvement of flint glaſs; and I was convinced that the propereſt and ſimpleſt means for rendering flint glaſs fit for conſtructing large achromatic teleſcopes conſiſt in removing the threads by means of a glaſs-cutter's wheel. When theſe faults are removed, the glaſs is to be kneaded in an oven and under a muffle, in ſuch a manner as to give it almoſt the form and ſize of the object-glaſs required to be made. This proceſs is amply deſcribed in a work which I publiſhed in 1783, entitled *Recueil de Memoires ſur la Mechanique et la Phyſique*.

It was in this collection that I ſaid that Euler had been the firſt who thought of correcting the aberrations of refrangibility by uſing ſubſtances of different refractive powers. Maupertuis engaged to get conſtructed at Paris Euler's

ler's object-glaſs, with water and glaſs like that which Newton had imagined, to render the aberration of ſphericity as little as poſſible; but this object-glaſs did not ſucceed, on account of the proportion ſince known between the refraction and diſperſion of common glaſs and water. This proportion is by the diaſporameter as 155 to 133 in regard to refraction, and as 100 to 67 in regard to diſperſion. But the ſtrong curvatures which muſt be given to deſtroy the aberration of refrangibility would occaſion a very ſtrong aberration of ſphericity. Thus, what might be gained on one hand would be infallibly loſt on the other. This was ſufficient to make Euler's project miſcarry, and to confirm the aſſertion of Newton, which tended to deſtroy all hope of being able to obtain achromatiſm in glaſs. Since that epoch the firſt knowledge of the ſtrong diſperſion of glaſs in the compoſition of which there is much lead, and which in England is known under the name of *flint glaſs*, has been aſcribed to John Dollond. This appellation however is not that beſt ſuited to this kind of glaſs. But what is of moſt importance to be known here is, that the more lead, or rather minium, is employed in making glaſs, the more will its diſperſive power be augmented. It was in the year 1759 that Dollond preſented achromatic glaſſes, compoſed of flint and crown glaſs. He ſays in his paper printed in the Philoſophical Tranſactions, that he found means to deſtroy with great eaſe the aberrations of refrangibility; but he confeſſes that he was ſtopped by an object more difficult to be ſurmounted, that of annihilating at the ſame time the aberration of ſphericity. It was thought, and is ſtill believed in France, that John Dollond was the inventor of achromatic teleſcopes; but we know that, ſome years after, Dollond's patent was attacked, on the ground that he was not really the inventor*.

* The author here goes on to give what he conſiders a correct account of the origin of the invention; but as he is incorrect in almoſt every circumſtance,

cumſtance, we ſuppreſs the reſt of his paper, and ſubjoin an article on the ſubject which appeared in the Gentleman's Magazine for October 1790, and which, we have been informed by Mr. Ramſden, contains a true ſtatement of the facts.

"The inventor was Cheſter More Hall, Eſq. of More Hall, in Eſſex, who about 1729, as appears by his papers, conſidering the different humours of the eye, imagined they were placed ſo as to correct the different refrangibility of light. He then conceived, that if he could find ſubſtances having ſuch properties as he ſuppoſed theſe humours might poſſeſs, he ſhould be enabled to conſtruct an object-glaſs that would ſhow objects colourleſs. After many experiments, he had the good fortune to find theſe properties in two different ſorts of glaſs; and by forming lenſes made with ſuch glaſs, and making them diſperſe the rays of light in contrary directions, he ſucceeded. About 1733 he completed ſeveral achromatic object glaſſes (though he did not give them that name) which bore an aperture of more than $2\frac{1}{2}$ inches, though the focal length did not exceed 20 inches; one of which is now in the poſſeſſion of the Rev. Mr. Smith, of Charlotte-ſtreet, Rathbone-place. This glaſs has been examined by ſeveral gentlemen of eminence and ſcientific abilities, and found to poſſeſs the properties of the preſent achromatic glaſſes.

"Mr. Hall uſed to employ working opticians to grind his lenſes; at the ſame time he furniſhed them with the radii of the ſurfaces, not only to correct the different refrangibility of rays, but alſo the aberration ariſing from the ſpherical figures of lenſes. Old Mr. Baſs, who at that time lived in Bridewell Precinct, was one of theſe working opticians, from whom Mr. Hall's invention ſeems to have been obtained.

"In the trial at Weſtminſter-hall about the patent for making achromatic teleſcopes, Mr. Hall was allowed to be the inventor; but Lord Mansfield obſerved, that "it was not the perſon who locked up his invention in his ſcrutoire that ought to profit by a patent for ſuch invention, but he who brought it forth for the benefit of the public." This, perhaps, might be ſaid with ſome degree of juſtice, as Mr. Hall was a gentleman of property, and did not look to any pecuniary advantage from his diſcovery; and, conſequently, it is very probable that he might not have an intention to make it generally known at that time.

"That Mr. Ayſcough, optician on Ludgate-hill, was in poſſeſſion of one of Mr. Hall's achromatic teleſcopes in 1754, is a fact which at this time will not be diſputed."

XII. *A Poſtſcript to Mr.* VARLEY's *Paper on the Methods of hardening and tempering Steel.*

ON looking over the paper I ſent for the laſt Number of the Philoſophical Magazine (ſee p. 92), I ſee that I omitted mentioning an experiment or two connected with the ſubject, which I meant to have introduced into it.

As ſteel is always found more compact and ſtrong bodied when hardened with a low heat, and as that effect is beſt obtained the colder the water is which is employed in hardening it, provided the water is clean, a circumſtance which ſhould always be attended to, it appeared probable, that if water was cooled down to the freezing point, or even lower, which it may be, and retain its fluidity, by being kept in a ſtate of perfect reſt, the effect might be heightened. I cauſed a large heap of ſnow to be collected together at a time when the thermometer ſtood at 22° of Fahrenheit, and making a deep hollow in the middle, I ſet a glaſs of clean water in the bottom of the hollow, and covered the whole with a board to prevent the air from diſturbing or cauſing any motion in the water. I heated ſome pieces of ſteel in the breech end of a gun barrel to a low red heat; and by means of an aſſiſtant to take off the board at the inſtant I arrived with the heated barrel and its contents, I quickly dropped the pieces into the water; which having ſtood all the preceding night in the ſituation above deſcribed, muſt, though ſtill fluid, have been cooled down to the temperature of the ſurrounding ſnow, which was ſtill found to be 22°. Upon taking them out, I found the pieces hard but brittle, having the appearance of ſteel that had been overheated.

Being diſappointed in what I had expected, I intended to repeat the experiment with a ſtill lower heat; but an alteration in the ſtate of the air prevented me from proſecuting the experiment at that time, and having ſince ſucceeded in

making

making uſe of quickſilver inſtead of water, I have never reſumed the experiment. As methods are now well known by which water may be cooled below the freezing point, even by the fire-ſide, and at an eaſy expence, ſome one who has time may perhaps think the experiment worth repeating: it might caſt ſome light upon the ſubject of hardening ſteel, and lead to ſome uſeful reſults.

The diſh I employ for tempering is very ſimple (A fig. 3, Plate IV.) and may be of any ſize, with a proportioned handle. It is made of plate-iron, with an edge turned up on every ſide a ſufficient height to hold tallow or oil enough to cover the pieces to be tempered. The corners are nipped together, and then folded up againſt the ſides, as repreſented in the figure; by which means they are prevented from letting out the oil.

The other pan or diſh B, turned up on three ſides only, is uſed to lay pieces intended to be hardened: a cover is of advantage where the pieces are intended to come out clean and bright, as it more perfectly defends them from the air, and of courſe prevents oxidation from taking place. By this inſtrument the work, though ever ſo ſmall or ſlender, may be brought out of the fire and dropped into the water with very little loſs of heat. It ought to be made of ſtronger plate-iron than is neceſſary for the other pan, and the corners cut out ſo that the ſides may bend up more ſquare. They need not be folded as in the other, not being intended to hold any liquid ſubſtance. Such articles as ſmall drills, pendulum and other ſmall ſprings need not be dropped into water, but only made to paſs through the air by toſſing them out and letting them fall to the ground, which will make them hard enough for moſt purpoſes.

Small drills may be hardened by holding their points in the flame of a candle, and, when ſufficiently hot, ſuddenly plucking them out: the air will harden them; and they may then be tempered, by taking a little of the tallow upon their point, and then paſſing them through the flame at about

half an inch above the point, and holding them there till the tallow begins to ſmoke. This method, known to all watchmakers, may be of uſe to other artiſts, and therefore not unworthy of the notice I have taken of it.

XIII. *Account of M.* FABBRONI's *Diſcovery of a Method to imitate the Floating Bricks of the Ancients* *.

PLINY mentions two cities in Spain, Maſſilua and Calento, in which a kind of bricks were made that would float in water. It is certain that Pliny does not here ſpeak of hollow bricks; for theſe would have had too little ſtrength for building. He muſt then mean ſolid bricks, but compoſed of a ſubſtance ſufficiently porous and light to float in water. Theſe bricks, according to Poſidonius, were made of a kind of argillaceous earth, which was employed to clean ſilver plate. As this earth could not be our tripoli, which is much too heavy to float in water, M. Fabbroni thought that it might be that earth called commonly *lac lunæ*, foſſil meal, mineral agaric, and guhr: this earth is abundant in Tuſcany, and is there actually employed for cleaning plate.

Guhr is liquid, and the *lac lunæ* approaches near to the mineral and calcareous agaric. The foſſil meal, according to M. Fabbroni, is a mixt earth which exhales an argillaceous odour, and throws out a light whitiſh ſmoke when ſprinkled with water. It does not effervеſce with acids; is infuſible in the fire, in which it loſes an eighth part of its weight, though it becomes ſcarcely diminiſhed in bulk; and according to the analyſis made by M. Fabbroni, conſiſts of the following component parts: ſiliceous earth 55, mag-

* Extracted from a ſmall work of 24 pages octavo, publiſhed at Venice, laſt year, under the following title: *Di una ſingolariſſima ſpecie di mattoni oſſia ritrovamento degli antichi mattoni gallegianti, diſſertazione letta nella publica adunanza dei Georgofili di Firenze, l'anno* 1791, *dal Signor* FABBRONI.

neſia

neſia 15, water 14, argil 12, lime 3, iron 1. With this earth, which is found near Caſteldelpiano in the territories of Sienna, M. Fabbroni compoſed bricks which, either baked or unbaked, floated in water. The baked were in nothing different from the unbaked, but in the ſonorous quality which they acquired. Theſe bricks reſiſt water exceedingly well, and unite perfectly with lime: they are ſubject to no alteration either by heat or cold; and about a twentieth part of argil may be added with advantage to their compoſition, without depriving them of the property of floating. M. Fabbroni tried their reſiſtance, and found it very little inferior to that of common bricks; but it is much greater in proportion to their lightneſs. One of theſe bricks, ſeven inches in length, four and a half in breadth, and one inch eight lines in thickneſs, weighed only $14\frac{1}{4}$ ounces; whereas a common brick weighed five pounds $6\frac{3}{4}$ ounces.

Foſſil meal is abundant in Tuſcany. The method of making theſe bricks differs little from that of common bricks: one man may make about a hundred of them in a day. They may be of very important benefit in the conſtruction of reverberating furnaces; as they are ſuch bad conductors of heat, that a perſon may bring one half of them to a red heat, while the other is held in the hand. They may be employed alſo for buildings that require to be light; for conſtructing cooking places on board ſhips; and alſo floating batteries, the parapets of which, if made of theſe bricks, would be proof againſt red hot bullets; and laſtly, for conſtructing powder magazines. The turrets which were raiſed on the ſhips of the ancients, adds M. Fabbroni, were perhaps formed of ſuch bricks; and perhaps they were employed in the celebrated ſhip ſent by Hiero to Ptolemy, and which carried ſo many buildings, conſiſting of porticoes, baths, halls, &c. ornamented with jaſper and agates arranged in moſaic.

XIV. *Experiments to determine how far Charcoal is a Non-Conductor of Heat. By Cit.* GUYTON. *Read in the Nat. Inſt. Germinal 6, An. 6. From the* Annales de Chimie, *Vol. XXVI.*

FROM the reſearches of chemiſts reſpecting the matter of heat, the different ſtates in which it is found, and the various ways in which it affects bodies, it is well known that charcoal is one of the weakeſt conductors of it. This obſervation has given riſe to the idea of forming furnaces with a double wall, in order that the intermediate ſpace may be filled with charcoal; and lately a very happy application has been made of this property to veſſels deſtined for forming and preſerving warm infuſions *.

I, however, do not know that any one has endeavoured to fix, at leaſt by comparative experiments, the limits of this kind of inſulation †. They have remained ſo vague that the ableſt chemiſts have not thought proper to pay any attention to them in judging of the reſult of their operations; and thus the celebrated Klaproth, in a ſeries of experiments on the alteration which ſtones experience in the ſtrongeſt fire, ſeems to think that the heat was almoſt equal for thoſe contained in crucibles of porcelain and thoſe encloſed on all ſides by charcoal. It is in a crucible of charcoal that chemiſts attempt the reduction of the oxyds of tungſten, titanium and uranium, though it is well known that what is ſought is a degree of heat ſufficient for their fuſion, and that every means poſſible are ſought for, in other reſpects, to expoſe them to a greater heat.

Theſe reflections induced me to think that it might be of ſome importance to give a more exact ſtandard of this in-

* It has been alſo propoſed, and certainly would be found to anſwer in practice, to ſurround bodies, intended to be kept cool, with charcoal—ice-houſes above ground for inſtance. EDIT.

† Some of the ingenious experiments of Count Rumford have been directed to this very object. See Eſſay viii. EDIT.

ſulating

ſulating effect of charcoal. The experiments which I made on this ſubject are as follows:—Having taken from the ſame box, two of Wedgewood's pyrometrical pieces perfectly ſimilar, I put one, which I ſhall call A, into a crucible filled with pure ſiliceous ſand, well dried. The crucible was eight decimetres in height, and ſix decimetres in diameter at the top. The piece of clay was placed in the middle, and the cover was luted on. The other piece B was placed in a covered crucible of the like ſize, but filled with charcoal duſt which had been previouſly expoſed to a red heat. The two crucibles were placed cloſe to each other on the grate of a large fuſing furnace, and expoſed to the action of the heat for about three quarters of an hour. When the crucibles had cooled, the piece A was taken from the ſand, and applied to Wedgewood's pyrometrical ſcale: it had experienced a contraction of 89 degrees. The piece B, when taken from the charcoal, ſtood at 60·25 degrees. It had aſſumed a greyiſh tint, but without any appearance of incruſtation. It thence reſults that the tranſmiſſion of heat through ſand is to that of heat through charcoal as 3 to 2 *.

The more the difference was ſtriking, the more it was neceſſary for me to employ precautions, that I might not be deceived by foreign circumſtances. It was poſſible that the

* This is not ſtated with that accuracy uſually diſplayed by GUYTON It leads to a concluſion, that the reſult would have been the ſame had the pieces been continued in the furnace for three hours, three days, or three weeks, inſtead of *about* three quarters of an hour, which is an unwarrantable inference. This experiment only proves, and that is a matter of ſome conſequence, what might have been fairly inferred from the known properties of charcoal, that to bring bodies ſurrounded with that ſubſtance to the ſame degree of heat with others ſurrounded with better conductors, *a longer time muſt be employed.* The not attending to this circumſtance is the cauſe why chemiſts, able ones too, often fail in difficult reductions where charcoal is neceſſarily put round the ore to take up the oxygen. EDIT.

pyrometrical piece might have ſome fault either in its compoſition or fabrication, which might change its diſpoſition to contract equally and proportionably to the heat. To obviate theſe doubts, the two pieces A and B were ſhut up in one crucible filled with ſand; ſo that they were diſtant from each other only about ſeven or eight millimetres, and the crucible was expoſed for half an hour to the moſt violent heat of a three-blaſt wind furnace. When the crucible cooled, it was found ſo much vitrified that there was a fiſſure in one of its ſides; but the ſand had not been deranged within it.

The piece A applied to the pyrometrical ſcale gave 163·5 degrees; it weighed no more than 1·491 grammes; and its ſpecific gravity was 2·232.

The piece B gave by the pyrometer 160 degrees; it weighed 1·53 grammes; and its ſpecific gravity was 2·346. It had loſt almoſt entirely the grey tint it had aſſumed in the charcoal, and diſtinguiſhed itſelf only by a black vitreous point, produced viſibly by the acceſſion of ſome foreign matter.

I did not expect, I confeſs, ſo complete a ſucceſs in this proceſs. The ſmall difference of $3\frac{1}{2}$ degrees is nothing when we conſider that the piece firſt ſhut up in the charcoal, and which ſtood at 60, was ſtill ſuſceptible of being contracted a hundred degrees more. It is known, beſides, that it is phyſically impoſſible that two bodies placed in the ſame crucible, in contact with the ſame matter, ſhould be exactly in the ſame condition to receive the heat, eſpecially when the wind is conducted in three directions neceſſarily unequal. A proof of this is furniſhed us by the vitrified fuſion being more advanced on one of the ſides of the crucible*.

* This way of accounting for the difference of $3\frac{1}{2}$ degrees might perhaps be admitted, if it could be ſhown that a longer expoſure to the heat would not have removed it. EDIT.

We

We may therefore conclude from theſe experiments*, that at the ſame heat a body encloſed in charcoal does not receive but about two-thirds of the heat of a body ſurrounded by quartzeous ſand; that the reduction of metals, which do not melt but at a heat of 130 degrees, cannot be effected in charcoal; that pyrometrical pieces do not enable us to judge with correctneſs of the heat communicated, but in ſo far as they are in contact with ſubſtances ſimilar to thoſe which ſurround the bodies ſubjected to it, or with bodies equally conductors; laſtly, that with theſe alterations we may make an advantageous uſe of this inſtrument.

NEW PUBLICATIONS.

Atti della Real Societa Economica di Firenze oſſia de' Georgofili. Tranſactions of the Royal Economical Society of Florence, &c. Vol. II. 1795. Vol. III. 1796. 8vo.

IN the ſecond volume of this work, after a continuation of the hiſtory of the Society, the following account is given of the life and ſervices of Profeſſor John Targioni Tozetti and of Saverio Manetti. The former was born at Florence, where his father was a phyſician, on the 12th of September 1712. In the year 1734 he received the degree of doctor, and ſoon after was appointed profeſſor. He purchaſed the library and manuſcripts of Micheli, his tutor, who died in 1737, with an intention of publiſhing a complete collection of the latter. He was afterwards named inſpector of the public library, from 110 manuſcripts, in which he publiſhed the letters of different learned men to Magliabechi; but, for want of encouragement, only five volumes were printed. Beſides his well-known Travels, he intended to

* For the reaſon given in a former note, the author's concluſions cannot be admitted. Nay, they are quite erroneous. He has not at all attended to the only fact proved by his experiments—that a given quantity of heat will not paſs in the *ſame time* through bodies poſſeſſing *different conducting powers*. EDIT.

have

have undertaken a work entitled, *Corographia e topographia fiſcia della Toſcana*, the plan of which was printed in 1754; but this great work was never completed. He died on the 3d of January 1783, leaving behind him a daughter and a ſon, Ottaviano Benedetto, preſent profeſſor of medicine.—Manetti was born on the 12th of November 1723; obtained the degree of doctor in 1747; and practiſed as a phyſician. He publiſhed that elegant work *Storia Nat. degli Uccelli*, and in 1770 began *Magazzino Toſcano*, which, after it had increaſed to 31 volumes octavo, was in the year 1777 continued to nine volumes more, under the title of *Nuovo Magazzino*. He died of an apoplexy on his birth-day, in the year 1784.—Of the papers in this work the following are the moſt worthy of notice: A ſhort account of the preparation of thoſe raiſins without ſtones, known under the name of Corinthian (currants), a production of the iſlands of Zante and Cephalonia, the greater part of which are purchaſed by the Engliſh and the Dutch. Experiments made by a Greek, in the neighbourhood of Piſa, ſeem to ſhow that the vines which produce theſe grapes may be cultivated with advantage in Tuſcany.

Gherardi, a Camaldule monk, has made ſome experiments to improve the culture, &c. of Spaniſh broom *(Spartium junceum)*, which muſt not be cut, as is commonly done, in January, but in October. Dr. Menaubone recommends the uſe of the common germander *(Teucrium chamædrys)* inſtead of cinchona, and the fruit of the privet *(Liguſtrum vulgare)* for dyeing. On the danger of employing the blue chickling vetches *(Lathyrus ſativa)*, which, as well as the dwarf chickling vetches, *Lathyrus cicera*, and the officinal tares, or true bitter vetches *(Ervum ervilia)*, are prejudicial when eaten, for a long time, in large quantities. Cautions againſt the uſe of a muſhroom, *Fungus alloides annulatus* of Vaillant, *Fung. raphanum redolens* Micheli, of which an engraving is given. Some experiments of Fabbroni ſeem particularly intereſting. As the farmers are

accuſtomed

accuſtomed ſometimes to divide the ſeeds of pulſe, and particularly beans, each half of which they place in the earth and obtain rich crops; he pounded coarſely ſome grains of corn, threw them into water, and ſaw that the particles which contained germs fell to the bottom. He ſowed theſe particles, and obtained as good plants as from the whole grains: many of them even produced a greater number of ſtalks. According to Fabbroni's experiments, the germ conſiſts entirely of the gelatinous or animal part, called by ſome the *gluten* or *colla*. In his opinion, the fermentation neceſſary in the manufacturing of ſtarch, is for the purpoſe of bringing the animal part to a ſtate of corruption, that it may be then ſeparated with the greater eaſe. Hence ariſes the unſupportable and certainly unhealthful ſtench which proceeds from a ſtarch manufactory. He aſks, therefore, if it be not poſſible to ſeparate the gelatinous part of the ſeeds by mechanical means, without uſing water as he did, by which the labour would be accelerated and the ſtench in part prevented. The ſeparated part might then be ground, and employed with other meal for baking bread. The germ, in regard to weight, forms only the ſixth part of the grain. Dr. Molinelli ſhews, by experiments, that it is better to warm olives in the ſun, than to ſuffer them, according to the old method, to ferment. Olives which in 1789 had been froſt-bitten, yielded, however, by the above method, good oil.

The third volume contains an account of the cultivation of cotton in the Iſland of Malta; a natural hiſtory of the neighbourhood of Piſa; a paper by Dr. G. Leſſi on the pernicious tendency of prohibiting foreign manufactures; Seſtini on the uſe of ſeſamum oil in the Levant. The two ſpecies of Linnæus, and the *Seſamum trifoliatum* of Miller, are, according to his aſſertion, only varieties. Targioni Tozetti junior has obſerved, that duck-weed *(Lemna)* in places where it totally covers the water, retires, or contracts itſelf, as ſoon as a branch of the *Rhus radicans* is dipped

dipped into the water near it. If a bit of the leaf-ſtalk of the latter be thrown into the water, it moves itſelf for ſome time in all directions, while an oily matter iſſues ſometimes from one end and ſometimes from the other; which, without doubt, is the cauſe of its movement. A like motion is obſerved in other plants which emit a milky juice: the *Schinus molle* exhibits this phenomenon ſtrongeſt next to the *Rhus*.—Experiments on obtaining ſugar from grapes. Propoſals for preſerving lemons from froſt. A Diſſertation by Dr. Palloni on the influence of plants to improve the atmoſphere: Collections by the ſame author, to prove the variation of climate in the ſouthern part of Europe. Inſtances of the great age of vines: one is mentioned of 112 years, and ſeveral of two hundred. Gum elaſtic diſſolved in rock oil or naptha produces an excellent varniſh for leather wine-bags, and other things; but it does not readily dry. Experiments on raiſing rhubarb in Tuſcany.

Von dem Perkiniſmus, oder den Metallnadeln des Dr. PERKINS *in Nordamerika, nebſt Amerikaniſchen Zeugniſſen, und Verſuchen Kopenhagener, &c.*—On Perkiniſm, or the Metallic Tractors of Dr. PERKINS of North America, with American Teſtimonies, and Experiments of the Phyſicians at Copenhagen. Publiſhed by M. HERHOLDT Surgeon, and Aſſeſſor KAFN. Tranſlated from the Daniſh; with the Obſervations of Dr. J. C. TODE, Profeſſor and Phyſician to the Court. Copenhagen, 1798. 8vo. 108 pages.

Profeſſor Schumacher at Copenhagen made experiments with tractors of braſs and iron on ten patients in Frederick's hoſpital at Copenhagen. He tried alſo tractors of ebony and ivory, which are ſaid to have cured a pain in the knee; with others of ſilver and zinc; and ſome of copper and lead. By the two laſt, pains in the knee, arm, and face are ſaid to have been mitigated. According to M. Klingberg's experiments, this remedy was of uſe in *malum iſchiaticum*; and according

according to thoſe of M. Steffens, in *malum iſchiaticum* and megrim. According to M. Bang, the pains in ſome caſes were increaſed, and in others allayed. According to M. Blech, the tractors were of uſe in *hemicrania* and gouty pains in the head; and, according to M. Hahn, in rheumatic pains in both ſhoulders. The principal document in this collection appears to be a letter of Profeſſor Abilgaard, in whoſe opinion Perkins's tractors will never acquire much value in medicine, and ſcarcely even have the merit of being a palliative; but, in a phyſical point of view, he thinks they deſerve the attention of phyſicians, and particularly of phyſiologiſts. Mankind, he ſays, hitherto have paid too little attention to the influence which electricity has on the human body; otherwiſe they would know that the effects produced on it by our beds is no matter of indifference. If the feather beds and hair mattreſſes, &c. are perfectly dry, the perſon who ſleeps on them is in an inſulated ſtate; but the contrary is the caſe if they are moiſt. He three times removed a pain in the knee, by ſticking the tractors, one on each ſide of the knee, ſo deep through the ſtockings that the points touched the ſkin. He removed a rheumatic pain in the head from a lady by the ſame means. M. Kafn, by the tractors, relieved, in others, gouty pains of the head, and megrim; and in himſelf, a rheumatic pain of the back, which, according to his ſenſations, was like a conſtriction in the cellular tiſſue. M. Herholdt, from his experiments, conſiders the effect of the tractors as indefinite and relative as that of other remedies. He, however, ſaw relief given by them in the ſtrangury in a caſe of ſyphilis. M. Bang alſo, at Soroe, freed a man from a violent gouty pain in the thigh by drawing the tractors 200 times over the affected part. M. Jacobſen likewiſe found benefit derived from theſe tractors ſeveral times in the common hoſpital at Copenhagen. M. Tode tried them alſo in rheumatic pains, tooth-ache, inflammation of the eyes, and obſerved that they neither did good nor harm.

According to the editor, the tractors act as a mechanical ſtimulus,

ſtimulus, as conductors of electricity, as galvaniſm, and alſo by the effects of the imagination.

Philoſophical Tranſactions of the Royal Society of London, for the Year 1798. Part II.

This part, which was delivered to the members a few days ago, contains the following papers:

A Diſquiſition on the Stability of Ships. By George Atwood, Eſq. F. R. S.—Quelques Remarques d'Optique, principalement rélatives à la Reflexibilité des Rayons de la Lumiere. Par P. Provoſt, Profeſſeur de Philoſophie à Geneve, &c. &c. &c. Communicated by Sir Charles Blagden, Knt. F. R. S.—An Account of the Oriſice in the Retina of the Human Eye, diſcovered by Profeſſor Sœmmering. To which are added, Proofs of this Appearance being extended to the Eyes of other Animals. By Everard Home, Eſq. F. R. S.—A Deſcription of a very unuſual Formation of the Human Heart. By Mr. James Wilſon, Surgeon. Communicated by Matthew Baillie, M. D. F. R. S.—Account of a ſingular inſtance of Atmoſpheric Refraction. In a Letter from William Latham, Eſq. F. R. S. and A. S. to the Rev. Henry Whitfield, D. D. F. R. S. and A. S.—Account of a Tumour found in the Subſtance of the Human Placenta. By John Clarke, M. D. Communicated by the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—On the Roots of Equations. By James Wood, B. D. Fellow of St. John's College, Cambridge. Communicated by the Rev. Nevil Maſkelyne, D. D. F. R. S. and Aſtronomer Royal.—General Theorems, chiefly Poriſms, in the Higher Geometry. By Henry Brougham, jun. Eſq. Communicated by Sir Charles Blagden, Kt. F. R. S.—Obſervations of the Diurnal Variation of the Magnetic Needle in the Iſland of St. Helena; with a Continuation of the Obſervations at Fort Marlborough, in the Iſland of Sumatra. By John Macdonald, Eſq. In a Letter to the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—On the Corundum Stone from Aſia.

By

By the Hon. Charles Greville, F. R. S.—An Inquiry concerning the Chemical Properties that have been attributed to Light. By Benj. Count of Rumford, F.R.S. M.R.I.A. —Experiments to determine the Denſity of the Earth. By Henry Cavendiſh, Eſq. F. R. S. and S. A.—An improved Solution of a Problem in Phyſical Aſtronomy; by which ſwiftly-converging Series are obtained, which are uſeful in computing the Perturbations of the Motions of the Earth, Mars, and Venus, by their mutual Attraction. To which is added an Appendix, containing an eaſy Method of obtaining the Sums of many ſlowly-converging Series which ariſe in taking the Fluents of binomial Surds, &c. By the Rev. John Hellins, F. R. S. &c.—Account of a Subſtance found in a Clay-pit; and of the Effect of the Mere of Diſs upon various Subſtances immerſed in it. By Benjamin Wiſeman, of Diſs in Norfolk. Communicated by John Frere, Eſq. F. R. S. With an Analyſis of the Water of the ſaid Mere. By Charles Hatchett, Eſq. F. R. S. In a Letter to the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—A Catalogue of Sanſcrita Manuſcripts, preſented to the Royal Society by Sir William and Lady Jones. By Charles Wilkins, Eſq. F. R. S.—A Liſt of Preſents to the Society, and an Index.

Caſes of the Diabetes Mellitus, with the Reſults of the Trials of certain Acids and other Subſtances in the Cure of the Lues Venerea. By JOHN ROLLO, *M. D. Surgeon General, Royal Artillery. Second Edition, with large Additions.* 1 *Vol.* 8*vo.*

This edition, which has been anxiouſly looked for by the medical world for ſome time, made its appearance a few days ago. It contains many intereſting recent communications reſpecting the Diabetes, and the additional trials of the new remedies in the Lues. The obſervations introduced in the firſt edition on ſeveral diſeaſes apparently ariſing from ſtomach affection, and including the application of the new doctrines of chemiſtry, as well as the deſcription of a morbid

poiſon

poiſon formed on ſores, have been omitted in the preſent, to reduce the ſize, and conſequently the price. They are ſtill, however, as the Preface informs us, to be conſidered as equally intereſting, and will continue to be proſecuted.

The experiments of Mr. Cruickſhank, an accurate and ingenious chemiſt, on the difference between the ſugar of milk and that of diabetic urine, and on urine and ſugar, are highly intereſting. While they in ſome meaſure unfold the peculiar nature of each, they alſo aſſiſt in the general explanation of diabetes, as well as what relates to the urine of patients labouring under other diſeaſes.

The reſults of the trials with the new remedies in the lues venerea (which have been ſubſtituted for mercury, and are believed to operate by introducing oxygen into the ſyſtem) completely eſtabliſh their efficacy in the ſecondary as well as in the primary ſtates of the diſeaſe. The oxygenated remedies have alſo been found efficacious in hepatic affections, (one of them attended with dropſy,) and in ſome caſes of remittent fever, the character of which was ſuch as would otherwiſe have demanded the exhibition of calomel.

It will not be eaſy for the oppoſers of the new medicines to get over the maſs of evidence brought forward in this work in their favour. To us there does not appear any way of trying the power of theſe medicines ſo fairly as in a military hoſpital, where the patients cannot deceive thoſe who preſcribe for them, but are *forced*, if neceſſary, to comply with the directions given them. To *pick out* a patient here and there from among a number of out-door patients, who, let the faculty preſcribe what they pleaſe, will only do what they themſelves like; and to bring forward caſes that have failed, under ſuch circumſtances, in being cured by the oxygenated medicines, is of all modes of proceeding the moſt uncandid, and muſt tend even to impreſs the public with an idea that a failure is *wiſhed for*, merely to defend a ſyſtem.

We cannot better cloſe the preſent article than by quoting the concluding words of Mr. Cruickſhank at the end of his

remarks

remarks on the effects of the new medicines.—"One of the two following positions must be allowed: Either these remedies cure the lues venerea, or, in 99 cases out of 100 the disease cures itself. Our opponents may take which side they choose; for, on either supposition, mercury must be unnecessary:—and this is our principal object."

An Inquiry into the Causes and Effects of the Variolæ Vaccinæ, or the Cow-pox. By EDWARD JENNER, *M.D. F.R.S. &c.* 4to.—*An Inquiry concerning the History of the Cow-pox, principally with a View to supersede and extinguish the Small-pox.* By GEORGE PEARSON, *M.D. F.R.S. &c.* 8vo.

These two interesting publications having avowedly the same humane and beneficial object in view, namely, the rooting out of a disease which has swept away an immensely greater number of victims from the world than the amount of all that have been destroyed by the plague or pestilence, there can be no impropriety in joining them together in this short notice; for, short it must be, as all we can aim at, or hope for, is to excite the curiosity of such of our readers as have not seen them, that they may examine the facts brought forward, and by this means be led to yield their individual assistance in the prosecution of an object of so much importance.

Opinions are not agreed as to the origin of this disease in cows; some supposing that they get it by infection from men milking them, who have been previously applying dressings to the heels of horses affected with the *grease*; while others, with more probability, consider it as being in the first instance compounded in the animal economy of the cow; after which, it may be conveyed to any number (in an obvious way by the hands of the milkers, as the seat of the disease is in the breast and teats, which are covered with eruptions similar to the small-pox). But its origin is of no importance:—its application to the benefit of mankind is what demands the greatest attention.

It is a ſingular fact, that it has been long known by country farmers and others, that this diſeaſe, which in ſeveral counties prevails at particular ſeaſons among the cows, and is often communicated to the hands of the milkers, exempts ſuch as have been ſo infected from being infected with the ſmall-pox.—It is ſtill more ſingular, that though they knew this fact, and alſo that no cow or human being had ever been known to die of the cow-pox, they never thought of having recourſe to a voluntary infection of this kind, to free themſelves and families from the poſſibility of being infected with the variolous poiſon, which ſo often proves mortal even when given by inoculation.

Theſe publications contain a great body of evidence, all tending to prove that perſons who have undergone the ſpecific fever and local diſeaſe occaſioned by the cow-pox, communicated either by accident to the hands when milking them, or by inoculation (for this has been already tried by the ingenious authors), are thereby rendered unſuſceptible of the ſmall-pox; and that matter from ſuch patients may be employed with the like effects, no difference being obſervable in the effects of the matter generated ſucceſſively in the firſt, ſecond, third, fourth or fifth human creature.

Pits from the ſmall-pox are a deformity that no one can certainly guard againſt even by inoculation. In the cow-pox no ſuch conſequences take place; for, though accompanied with fever, the puſtules are local, and the place may be choſen.

According to ſome obſervations, (but this is not quite certain,) the ſame perſon may repeatedly have the cow-pox. If ſo, practitioners may avail themſelves of this mean of exciting an innocent fever as a remedy of various diſorders; it being a truth, that fevers are occaſionally efficacious remedies for epilepſy, hyſteria, inſanity, St. Vitus's dance, tetanus, &c.

As the cow-pox poiſon acts upon the whole conſtitution in ſeven or eight days after its admiſſion, and the ſmall-pox moſt frequently not till fifteen or more; in caſes where ex-

poſure

poſure to the ſmall-pox infection is unavoidable, and the conſequence of infection at the time dangerous (as in pregnancy), inoculation with the former might, by its quicker action, produce that change in the ſyſtem which would inſure the leaſt deadly of the two maladies.

To urge to ſuch inquiries as may afford a ſufficient number of facts, from which to draw all the uſes that may be derived from a proſecution of this ſubject, ſeems to be the aim of the authors; and no ſagacity is required to predict, ſays Dr. Pearſon, that, ſhould the practice of inoculating for the cow-pox ever become very general amongſt young perſons (which we hope will be the caſe), the variolous infection muſt be extinguiſhed; and, of conſequence, that loathſome and deſtructive diſeaſe, the ſmall-pox, be known only by name. And this benefit will accrue, without even the allay of the introduction of a new diſeaſe; it being plain from the nature of the cow-pox poiſon, that (the other being once rooted out) it will be eaſy to avoid and prevent its diſſemination—as there muſt be at leaſt a real contact with it to cauſe infection.

Philoſophy of Mineralogy. By ROBERT TOWNSON, *L.L.D. F.R.S. 8vo.*

This is a valuable performance, and will be highly uſeful to chemiſts and mineralogiſts. It gives an account of the elementary ſubſtances which enter into the compoſition of minerals, followed by an enumeration of compounds arranged according to Dr. Babington's ſyſtem; intereſting information reſpecting ſtratification; the irregularities of the earth's ſurface, veins and petrifactions; the value and uſe of the external characters of minerals; a good nomenclature in Engliſh, Latin, and German, with their correſponding definitions—colour, figure, ſurface, &c. &c.; inſtructions for collecting ſpecimens; and a liſt of works on mineralogy.

This work is the outline of a larger formerly announced, but which did not meet with the neceſſary encouragement. We ſhall ſtill hope, however, to ſee it made public.

INTELLIGENCE

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

SWEDEN.

THE Royal Academy of Inſcriptions, the Fine Arts, Hiſtory, and Antiquities at Stockholm has propoſed the following as prize ſubjects for the preſent year:

HISTORY.

"Reſearches reſpecting the hiſtory of the herring-fiſhery, both on the coaſt of Scandinavia and all the coaſts of countries ſubject to the Swediſh government, from the reign of Guſtavus I. to the preſent time."—The prize is a gold medal of the value of 26 ducats.

FOREIGN LANGUAGES.

"A treatiſe, either in Latin or French, on the duty of an hiſtorian, when delineating the characters of great men, to examine carefully the character and genius of the century in which they lived, in order, on the one hand, that he may not give importance to and propagate principles prejudicial to ſociety; and on the other, that, by cenſuring the errors of great men, he may not weaken and leſſen that reſpect and admiration which are due to diſtinguiſhed talents, activity, reſolution, courage, and other heroic virtues, the exiſtence of which is ſo neceſſary for the independence and happineſs of nations."—The prize is a gold medal of the ſame value.

ANTIQUITIES.

"An inquiry into the origin, nature and object of the military expeditions and naval armaments of Sweden to the middle of the twelfth century."—The prize is a gold medal of the value of 15 ducats.

INSCRIP-

INSCRIPTIONS AND DEVICES.

"1. Hints reſpecting a Latin inſcription for the Exchange at Stockholm.

"2. Hints reſpecting medals to commemorate the moſt remarkable events which have taken place in Sweden in the preſent century; to be choſen at the option of the author." The prize is a gold medal of the value of 12 ducats.—Thoſe who wiſh to become candidates for this prize muſt ſend propoſals reſpecting both the above ſubjects, otherwiſe their works cannot be laid before the Academy for examination.

The papers on all theſe ſubjects muſt be tranſmitted to the Academy with devices, and accompanied with the names of the authors, each in a ſealed note, before the 20th of January 1799.

BERLIN.

The Society of the Searchers into Nature have propoſed the following queſtion, to be anſwered before the firſt of January 1800, as the ſubject of a prize:—"Allowing that electricity is requiſite for the production and formation of hail in the atmoſphere, can any hopes be entertained of rendering the clouds incapable of producing it, and of preventing its formation by means of conductors, ſuch as thoſe uſed for preventing the effects of lightning? what are the means to be employed for this purpoſe? and what obſervations and data, in general, have we on this head worthy of being taken into conſideration?"—The prize is 20 ducats.

GOTTINGEN.

ASTRONOMY.

In the month of December of the laſt year, Dr. Schroter of Lilienthal tranſmitted to the Royal Society at Gottingen ſeveral aſtronomical obſervations, the moſt important of which were the following:

Dr. Schroter obſerved at different times, and with the greateſt certainty, in Jupiter's four ſatellites, even in both the

ſmalleſt, the firſt and the ſecond, dark ſpots of an atmoſpheric nature, tranſitory and ſubjected to accidental changes. They however fully convinced him, that all theſe ſatellites each during the time of its ſynodical revolution revolves around its own axis: the caſe is the ſame with the ſatellites of Saturn. The ſpots of the latter cannot indeed be ſeen like thoſe of the ſatellites of Jupiter; but in all the five, particularly the firſt, ſecond, third and fifth, Dr. S. ſaw ſimilar and very diſtinct variations of light, at a conſiderable number of periods, which give him reaſon to ſuppoſe the exiſtence of ſpots of the like kind. They are not regular, as in the caſe of our moon, but accidental variable modifications; ſometimes ſubject to irregular changes, which, however, by their uſual, often very long, periodical duration, mark out certain regions expoſed to common atmoſpheric modifications, ſimilar, for example, to the tract of the monſoons on our earth. The moſt important reſult, however, is, that Saturn's ſatellites revolve round their axes during the time of a ſynodical revolution; a circumſtance which M. Lichtenberg conjectures in reſpect to all the ſecondary planets. Obſervations tending to confirm this ſuppoſition will be publiſhed by Dr. Schroter in the ſecond and third part of his Collections towards the neweſt aſtronomical diſcoveries.

PHYSIOLOGY.

Profeſſor Weideman at Brunſwick tranſmitted lately to the Royal Academy of Sciences a paper on the more exquiſite ſenſibility of certain parts of animals, from which the following is an extract in the author's own words;

"Many animals have a moſt exquiſite ſenſibility at the tip of the muzzle, and particularly thoſe which have theſe parts long. This delicate ſenſibility is principally produced by the end of the *nervus infraorbitalis*, in conjunction with ſome fibres of the optical nerves, eſpecially the middle branches, which run acroſs the *maſſeter* in an oblique direction. The *nervus infraorbitalis* is ſtrongeſt in thoſe animals which

which have long muzzles, or muzzles covered with long whiſkers. The muzzles of animals are moved in particular by three delicate muſcles; and, beſides theſe, ſeveral folds of the cutaneous muſcle lie at the ſide of the ſnout, and the branches of the optic nerves proceed in particular to theſe muſcles. I however found in a hedge-hog ſome fibres of the optic nerve proceed to the root *(bulbus)* of a briſtle. For the moſt part, however, theſe roots of the long whiſkers are connected with the numerous and proportionably ſtrong branches of the *nervus infraorbitalis*, which proceed to them in the form of a bundle, and in general tranſmit two fibres to the root of each briſtle, which they embrace on both ſides. This appearance I found exceedingly pretty in the head of a hare newly killed. In animals not provided with theſe whiſkers, the nerve proceeds only to the glandulous ſkin of the noſe, and which may be very diſtinctly obſerved in ſwine. The whiſkers ſerve as the vehicle of a finer ſenſation, in order to forewarn animals in certain circumſtances of the near approach of danger; for the ſlighteſt touching of the ſummits of theſe briſtles excites in them a very ſtrong ſenſation. Cats, and other animals which hunt for their prey in the night-time, extend the ſkin of the whiſkers, which enables them to aſcertain the nature, and even the hardneſs or ſoftneſs, of the bodies which they approach."

CHEMISTRY.

In the ſitting of Auguſt 4, M. Gmelin communicated to the Academy an account of ſome experiments which he made on the red lead of Siberia and the white gold ore of Fatzebay in Tranſylvania, and the new metals extracted from them. He found it eaſy to extract the metal of the former, which, on account of the beautiful colour it communicates to bodies combined with it, is by Vauquelin called chrome. He obtained it of an emerald green colour by means of the muriatic acid, and precipitated it from the latter by zinc, the Pruſſic acid, ſulphat of potaſh, carbonat of potaſh, and cauſtic

potaſh. By potaſh it was precipitated of a mountain blue colour; and, when fuſed under this form with ſalts, communicated to them a green colour. The ſlag alſo acquired the ſame when the latter was fuſed with charcoal powder, pounded glaſs, borax, or its acid. As it diſſolved, however, ſo eaſily in fluxing ſalts, he found it difficult to fuſe it by this method to a metallic button, till it was firſt precipitated by zinc from the fluid ſlag; and it then gave a button of a leaden grey colour, which, when melted with borax, communicated to it a green colour. Theſe characters ſufficiently diſtinguiſh it from molybdena, which Bindheim ſuppoſed it to be, as well as from arſenic, which Lehman ſought in it; though it agrees with both in this, that its calx readily aſſumes the properties of an acid, and with the laſt, that when burnt on charcoal by the blowpipe it emits a like ſmell.

The other ſeries of experiments were made with the ſo-called aurum problematicum, from which, after it had been well mixed with finely-pounded grains of quartz, the metal was extracted by frequent boiling in nitro-muriatic acid, and then precipitated by potaſh. The depoſit being boiled with cauſtic ley, what the latter diſſolved was again precipitated by an acid. What was then obtained melted very eaſily by the blowpipe upon charcoal, without any remarkable ſmell, but ſurrounded with a beautiful blue and green flame, into an almoſt tin-white, hard, very brittle button, the whole ſurface of which had a granulated appearance; and when longer expoſed to the heat it diſſipated in flame and ſmoke. It did not diſſolve entirely in pure nitric acid, but in the nitro-muriatic. What was precipitated from both exhibited the ſame phenomena by the blowpipe as the former metallic button. It is not precipitated by zinc and iron only, but alſo by copper. From theſe properties it appears to differ from all the metals hitherto known. Profeſſor Klaproth has given it the name of *Tellurium*.

FRENCH

FRENCH NATIONAL INSTITUTE.

In the public ſitting of Vendemiaire 15, An. 7, the following notice of the labours of the different claſſes was read by the ſecretaries of each claſs:

PHYSICAL SCIENCES.

The experiment of the reduction of lead by ſulphur having been announced by Wiegleb, a German chemiſt, as favourable to Stahl's theory of phlogiſton, Citizen Guyton ſhewed that this experiment is not concluſive, if an apparatus be uſed that permits the operator to keep an account of all the aëriform fluids. The ſame chemiſt made experiments on the nature of the ſuccinic acid and the products from its decompoſition. From his experiments he is inclined to aſſign to that foſſil a vegetable origin, which agrees with the obſervations of mineralogiſts. Citizen Guyton alſo laid before the claſs the products of his experiments on urinary concretions, which, as Scheele announced, manifeſt the preſence of a peculiar acid formed in theſe concretions.

New obſervations on the yellow colour that may be extracted from vegetables for the art of dyeing, were communicated by Cit. Chaptal. (See *Phil. Mag.* Vol. I. p. 430.)

The ſame chemiſt ſhewed that the difference between the acetous and the acetic acid conſiſts in the proportions of carbon, which is the radical; and that the acetous acid contains much more than the acetic.

A heavy greyiſh ſtone of a cloſe texture, found ſome years ago in the quarries of Menil-Montant, near Paris, and which had been conſidered for ſome time as ſulphat of barytes and afterwards as ſulphat of lime, was analyſed by Citizen Vauquelin. It appears to be a compound of the ſulphat of ſtrontian and the carbonat of lime.

The ſame chemiſt preſented two proceſſes for the ſeparation of the component parts of braſs by the wet way. The firſt conſiſts in diſſolving in the nitric acid a determinate quantity

quantity of this mixt metal, and precipitating the ſolution by potaſh added to exceſs. The ſecond, which the author prefers, is by diſſolving the mixture in the ſulphuric acid, and afterwards plunging in the ſolution a plate of zinc, which precipitates the copper in a metallic ſtate.

Cit. Teiſſier has ſhewn, that by forming artificial meadows the advantages of natural meadows may be much increaſed.

Reſearches on the nature of the ſoil of the iſland of Malta, by C. Teiſſier; on that of Egypt, by C. Bruguiere aſſociate; and on the ſtructure of Mount Perdu, the higheſt of the Pyrenees, by C. Ramond aſſociate, have occupied, in ſucceſſion, the claſs of the Phyſical Sciences. It received alſo ſome intereſting details in regard to the medical art on an extra-uterine pregnancy, obſerved by C. Saucerote aſſociate; on urinary and arthritic concretions, by the ſame; and the caſe of a man, all whoſe bones, except the teeth, acquired in the ſpace of a few years an exceſſive ſize, without the muſcles being affected in this ſingular malady.

For a long time naturaliſts have been ſenſible of the neceſſity of having a new method of claſſing birds; and they wiſhed that this method, by being ſuited, on account of its preciſion, to the great quantity of known ſpecies, ſhould render the characters of them eaſy to be diſtinguiſhed; and be at the ſame time applicable to other ſpecies not yet diſcovered. This has been executed by Cit. Lacepede. His new table, in which he has included 124 genera, is preceded by new principles, according to which, in his opinion, naturaliſts ought to compoſe methodical tables of the different claſſes of animals.

MATHEMATICAL AND PHYSICAL SCIENCES.

Analyſts divide the problems which they wiſh to reſolve into two claſſes, one of which includes thoſe which can be reduced to an equation, in which there is one unknown quantity combined, throughout all the arithmetical operations,

tions, with itſelf, and with the known quantities: theſe are called determinate problems. The indeterminate are thoſe, the ſolution of which depends on an equation where there are found two or more unknown quantities, ſuſceptible of all the values which the analyſt may aſſign to them, ſo that the unknown quantities remain indeterminate, at leaſt in certain limits. The proceſſes are very different in theſe two ſorts of problems, and hence ariſe two kinds of analyſis. Among thoſe queſtions which cannot be ſolved but by the indeterminate analyſis, are ſome reſpecting the nature of numbers; queſtions of a delicate nature and difficult to be treated, which require intenſe thought and exceedingly delicate and varied reſources of mind, but which, at the ſame time, excite the curioſity more, and become more engaging. Cit. Legendre publiſhed, in the Tranſactions of the Academy of Sciences for 1785, a memoir upon this ſubject. In the laſt quarter he has made public new reſults reſpecting his further reſearches in this branch of the analytic art, under the title of *Eſſai ſur la Théorie des Nombres*. This modeſt title promiſes much leſs than the work affords. It is a complete treatiſe of every thing known on the theory of numbers, or even of indeterminate analyſis. But theſe theories would be much leſs uſeful if they wanted the demonſtrations and new theorems diſcovered by Cit. Legendre.

A new work on determinate analyſis engages, at this moment, alſo the attention of analyſts. Cit. Lagrange has publiſhed a work on the reſolution of numerical equations. He gives this name to equations where, with the unknown quantity, there are only numbers which retain their numerical value. In the laſt analyſis every determined problem may be reduced to expreſſions of this kind. It is of great importance, therefore, to have methods given for reſolving them; and we might even be inclined to believe that it would be ſufficient for mathematicians to confine themſelves to reſearches for diſcovering theſe methods. But geometricians

tricians are not ſatisfied unleſs their reſults are more general. They deſire, for each degree, a final equation, which may repreſent in what manner each of the given numbers concurs to the different values of the unknown quantity; ſo that with theſe expreſſions, or formulæ, nothing remains to be gone through but ſimple arithmetical operations to determine the unknown part of an equation, whatever may be the abſolute values of the known quantities which enter into its compoſition. Hitherto we had none of theſe formulæ beyond the fourth degree. It was neceſſary then to recur to means for reſolving individually numerical equations, and to bring out, one by one, the values of the unknown quantity. Newton gave a method for this purpoſe; but it is only approximative, even when there exiſt exact values of what is ſought for, and in certain caſes it gives nothing.

In 1767 Cit. Lagrange, whoſe name is here naturally coupled with that of the Engliſh geometer, gave a new method, exempt from the inconveniences of that of Newton. Analyſis was then in a more advanced ſtate, and the author took advantage of thoſe degrees of perfection which he had himſelf given to the analytic art. The queſtion then is to keep the number ſought between two determined fractions, which go on decreaſing, one of which is greater and the other leſs than that number, however ſmall the fractions may be; and thus, by exhauſting a fractional difference, we arrive at exact values of the unknown quantity, if there are any poſſible, or at leaſt we approach ſufficiently near it. Theſe firſt reſearches of Cit. Lagrange are but a ſmall part of the volume juſt publiſhed. Thoſe which form this laſt work will add a new degree to the eſteem and gratitude to which he has been long entitled by his numerous and learned labours.

Cit. Duc Lachapel, an aſſociated member, read a memoir on an obſervation which he made at Montauban on the appulſe of the Moon and Mars. Aſtronomers have given this name to phenomena where the moon paſſes ſo near to

a ſtar,

a ſtar, or a planet, as almoſt to eclipſe it. They obſerve theſe phenomena with great care, becauſe they can deduce from them, with ſufficient preciſion, the errors in the lunar tables. The *Connoiſſance des Temps*, An. VI. announced an occultation of Mars viſible at Paris. The diſtance between Montauban and Paris was ſufficiently great for the Moon to appear in the latter a little higher than Mars, and conſequently not to produce an occultation of the latter. This obſervation gave Cit. Duc an opportunity of examining with attention the diſk of Mars; and he obſerved, in the auſtral part, a ſpot of a very ſenſible diameter, round and white, a colour very apparent on a planet the appearance of which is reddiſh. According to ſome other obſervations which he made afterwards, Cit. Duc eſtimates that this ſpot is ſituated at the pole of the planet.

A paper was read in the ſitting of Meſſidor 15, in which Cit. Delambre gave an account of the operations for meaſuring the firſt baſe from Melun to Lieuſaint, extending in length 11808·5 metres. A ſecond baſe has juſt been meaſured on the high road from Perpignan to Narbonne. The length was found to be 11702·6 metres. Rarely have baſes of ſuch a length been meaſured, and ſtill more rarely has it been found poſſible to place them on ground ſo ſmooth and ſo level. But however regular the roads choſen for theſe operations may have been, there occurred in them a bending almoſt imperceptible to the ſight, and which rendered it neceſſary to break the two baſes. In this manner, inſtead of meaſuring a ſtraight line for each baſe, two were meaſured, forming an angle of 180° of the ancient diviſion. The exceſs of the broken line over the ſtraight line to Melun did not exceed 27 centimetres, and not more than 6 at Perpignan. The new baſe was meaſured with the ſame rules of platina which ſerved at Melun. The difficulty of tranſporting, without any accident, inſtruments ſo delicate, along ſo lengthened a route as that from Paris to Perpignan, and the neceſſary preparations and local difficulties,

culties, prevented the commencement of the real meaſurement till the 19th of Thermidor. It was however finiſhed on the firſt complementary day. The weather was, at times, ſo tempeſtuous, that it rendered the obſervations more tedious. One day, in particular, it blowed with ſuch violence that it diſplaced the rules and carried them away, together with their ſupporters, notwithſtanding their weight and a conſiderable friction. The obſervers continued to ſtruggle againſt this obſtacle, but at length were obliged to yield, and even to begin again during calmer weather the labours of that day, which amounted to 240 metres. At length the labour being repeated, with every poſſible care, and under the moſt favourable circumſtances, the firſt meaſurement was found to be correct within a millimetre of 240 metres. This aſtoniſhing conformity may ſerve to give an idea of the preciſion which may be hoped for from rules, when employed under favourable circumſtances. At the moment when C. Delambre wrote theſe reſults, he was preparing to return to Paris with Cit. Mechain. Nothing therefore remains but ſome calculations to be made for determining the length of a degree of the meridian, in the preſence of the learned men deputed by different powers of Europe to aſſiſt in this grand operation.

MORAL AND POLITICAL SCIENCES.

Cit. Buache has publiſhed a general map of Guiana; C. Mentelle, an analyſis of his leſſons of geography and coſmography, with an eſſay on the hiſtory of the Hebrews; C. Koch, aſſociate, a work entitled *Sanctio pragmatica Germanorum illuſtrata*; and C. Anquetil, an expoſition of the motives of the wars and treaties of France, from the year 1648 to 1783.

Cit. Bouchaud read hiſtorical and critical reſearches reſpecting the law *Julia miſcella*. This law, the object of which was to favour marriage, allowed widows to enter a ſecond time into that ſtate, without loſing certain advantages attached,

attached, either by contracts or teſtamentary diſpoſitions, to their perſevering in a ſtate of widowhood. Cit. Bouchaud communicated alſo to the claſs reſearches on the formation of the Roman and municipal colonies. The colonies founded by the Romans were called Roman, Italian, or Military: but they did not all enjoy the ſame rights; and a like political inequality was remarked between the *municipia*, which Feſtus likewiſe diſtributed into ſeveral claſſes. The moſt favoured of the *municipia* was Tuſculum, the inhabitants of which obtained all the rights of Roman citizens. Cit. Bouchaud has written, on the ſame ſubject, three other memoirs, to explain the different ſyſtems of magiſtracy by which the *municipia* and colonies were governed.

In a memoir on the ſtate of the French marine, at the beginning of the 14th century, Cit. Legrand deſcribed the naval battle of 1304 between the French and the Flemings, a very particular account of which he found in a hiſtory, in verſe, entitled, *La Branche aux royaux lignages*, written in 1306 by William Guiart. This ſmall work, conſiſting of fifteen or ſixteen verſes, one of the oldeſt now extant on the hiſtory of the French navy, gives a very accurate deſcription of the naval tactics and manœuvres of that period. Cit. Legrand has employed it to make known the different kinds of veſſels of which ſquadrons were then compoſed, and the manner of fitting them out either for attack or defence. From this memoir it reſults that, until Francis I, the kings of France had no regular navy; and that, in their naval wars, they were accuſtomed to purchaſe or hire privateers, ready equipped and manned, or merchant ſhips, which they manned themſelves and furniſhed with warlike machines. This memoir is an extract of a Hiſtory of the Arts and Sciences in France, on which Cit. Legrand has been employed for ſeveral years. He is employed alſo in a Hiſtory of the French Language and Literature: and in both theſe

works

works he has made particular uſe of the manuſcripts in the National Library, with the care of which he has been entruſted. As ſeveral of theſe manuſcripts contain very curious paintings in miniature repreſenting real inſtruments, furniture, games, dreſſes and combats, Cit. Legrand propoſes to have accurate drawings made from them, in order that they may be engraved and added to his Hiſtory of the Arts.

Cit. Anquetil read a hiſtorical fragment on Denmark, being an abridgement of the Daniſh hiſtory from the royal law in 1660 down to the preſent time. This royal law has been the particular ſubject of a memoir by Cit. Deſales, who has chiefly applied himſelf to examine the part taken by the three orders of the ſtate in that famous act.

Cit. Deſales, in another memoir, gave an analyſis of a book, printed in 1582, under the following title: *Le Mirouer des François, contenant l'état et maniement des affaires de France, &c. Le tout mis en dialogue par Nicolas de Mentand.* The real name of the author, according to Lammonaie, is Nicolas Barnaud. The choice of the ſpeakers in this work, which conſiſts of dialogues, is very ſingular: they are Cham, Sem and Japhet, Nimrod, Tubal-Cain, and other perſonages of the like kind, who diſcuſs the affairs of France under Henry III; and who propoſe to convert the bells into money, to ſuppreſs the order of Malta, and to unite Belgium to France.

Cit. Papon read the preliminary diſcourſe to a hiſtory he has undertaken of the French revolution.

Cit. Rœderer, in a work entitled *L'Art de ſavoir ce qu'on dit en Politique et en Morale*, &c. propoſes to apply analyſis to a great number of queſtions. He made the firſt trial of his method on that propoſed by the claſs as the ſubject of a prize, viz. What are the inſtitutions beſt calculated to lay the foundation of morals in a nation? By examining and diſtinguiſhing the different acceptations of each of the words which compoſe the above queſtion, Cit. Rœderer found, by decompoſition, that it is ſuſceptible of 120 different meanings;

ings; and he afterwards determined, by recompoſition, that in which it ought to be underſtood*.

Cit. Dupont read two memoirs: one on the baſes of morals; and another on the philoſophy of Haller and Bonnet.

Cit. Bernardin Saint-Pierre traced out, in a fragment entitled *Le Matin du Jour de la Mort de Socrate*, one of thoſe immortal examples which may afford uſeful leſſons of morality. The accuſers of that philoſopher, apprehenſive of the inconſtancy of the Athenians, and fearing the regret, and, perhaps, the vengeance that would follow his death, but dreading above all his innocence, come and promiſe him his life, his liberty, and honours, if he will acknowledge himſelf guilty. When his ironical but ſolid anſwers deprive them of all hope of being able to perſuade him by their arguments, they introduce his family, and preſent to him his children, who wiſh to die with him, and with whom he ſheds tears. For a moment they ſuppoſe him overcome, becauſe they ſee him affected; but he replies to his enemies, "I weep for joy that I am about to leave behind me children worthy of their father." Theſe dialogues, in which the character of the perſonages is ſcrupulouſly preſerved; where Lycon ſpeaks as a ſophiſt, Anytus as a ſuperſtitious man, and Melitus as a politician, form the firſt act of a drama, which Saint-Pierre propoſes to end with the death of Socrates.

LITERATURE AND THE FINE ARTS.

Cit. Dupuits read a ſecond memoir on the Pelaſgi, a nation of whom ſcarcely any thing more is known than the name, and whoſe antiquity goes beyond the fabulous ages. The author places the origin of theſe people in Egypt; from which he endeavours to ſhow that the Pelaſgi ſpread into Lybia as far as the Atlantic ocean, and afterwards paſſed into Peloponneſus, the Archipelago and Aſia. Theſe conjectures of Dupuits reſult from a comparative view of the

* Does this propoſal ſhow a progreſſive improvement in literature? or, Are the French going back again to the jargon of the ſchoolmen? EDIT.

religious worſhip of the Pelaſgi, and that of the people in Upper Egypt and Ethiopia; as well as from the traditions and geographical names common to the Pelaſgic nations, the Egyptians, and the Ethiopians.

Cit. Langles has already conteſted with the Europeans the invention of the compaſs, of paper, and of printing, in order to aſſign them to the Orientals. In a new memoir on gunpowder he deprives the German monk, Berthold Schwartz, of the fatal honour of that terrible invention, and aſſerts that it was conveyed to us from the Arabs. He aſſures us, that they made uſe of it, in 690, at the ſiege of Mecca; and he adds, that the Arabs derived it from the Indians, among whom it was known in the remoteſt antiquity, ſince their ſacred books (the Vedam) forbid the uſe of it in war. Cit. Langles is of opinion, that a knowledge of theſe different inventions might have come to us from the Eaſt on the return of the cruſaders. There is an interval, however, of two centuries between the laſt cruſade and the firſt typographical attempts of Guttemberg in the city of Straſbourg about the year 1440. Gunpowder was known earlier in Europe than printing; but it does not appear that it was employed there in war before the battle of Creci, where the Engliſh had ſix pieces of cannon. If the conjectures of Citizen Langles are well founded, the Europeans, at preſent, only carry back to the Eaſt knowledge which we formerly borrowed from that quarter. Thus every thing changes on the face of the globe; the arts are loſt in one nation to be revived in another; nations themſelves are effaced and diſappear; and vaſt accumulations of water covered formerly thoſe countries which we inhabit at preſent. All this proves, that the ſmall globe upon which we reſide is very old; and that to live a century or two is nothing. We have ſcarcely time to commence our ſtudies.

Cit. Langles read alſo a memoir on the Arabian literature.

Cit. Bitaubé read a memoir entitled *Des Jugemens de quelques*

quelques Philoſophes de l'Antiquité ſur les Republiques anciennes. Second part. In this ſecond part the author confines himſelf to an examination of the opinions of Xenophon and Iſocrates reſpecting the republics of Sparta and Athens.

Cit. Ameilhon continues his reſearches on the art of dyeing among the ancients. His third memoir is particularly devoted to an examination of the ſubſtances from which the ancient dyers derived their red colours*. The ſcarlet red was procured from the *coccus*, which, as far as appears, was our *kermes*. This ſmall inſect is found on the thorny leaves and tender ſhoots of a kind of *Quercus ilex* or ever-green oak, which grows on the ſtony hills of Provence and Languedoc. The *purple* red, which was called ſimply *purple*, was the moſt valuable colour. It was reſerved for the veſtments of the firſt magiſtrates and of emperors. It was extracted from two ſmall ſea ſhell-fiſh, the *buccinum* and the *murex*. Reaumur found the former on the coaſts of Poitou; and Duhamel the latter on thoſe of Provence†.

Cit. Ameilhon read alſo a ſhort notice reſpecting a Greek manuſcript containing a work on the ancient chemiſtry, and erroneouſly aſcribed to Democritus of Abdera.

C. Camus read a memoir on a book, which at bottom contains nothing very intereſting, as it is only a bad romance, written in honour of the emperor Maximilian I. under the title of *Des hauts Faits d'Armes et des Avantures de l'illuſtre et célébre et belliqueux Heros et Chevalier du Tewerdank, ou du Grand Penſer.* C. Camus, in this memoir, examines the queſtion, which has been a ſubject of controverſy among the learned, whether this book was printed with engraved blocks of wood, or with moveable

* For ſome curious information on this ſubject ſee Beckmann's Hiſt. of Inventions, vol. ii. p. 101. EDIT.

† On this ſubject ſee Bancroft's excellent work upon the Theory of Permanent Colours. EDIT.

types. There are ſtrong reaſons for adopting the latter opinion.

Cit. Cailhava, always occupied with the comic art, communicated extracts from ſome ancient Spaniſh pieces of Calderon de la Barca, entitled *Le Purgatoire de St. Patrice, Le Diable et le Saint*, &c.; and he obſerves, that the dramatic authors, who bring upon the ſtage in ſo agreeable a manner ghoſts, devils, and monks, have not the merit of the invention, and only carry us back to thoſe coarſe farces which diſgraced the infancy of the modern theatres. Cit. Cailhava calls the attention of government to that noble dramatic art, which might and ought to exerciſe a powerful influence over the opinions and manners of civilized nations. The ancients had particular magiſtrates appointed for the purpoſe of keeping a watchful eye over the theatres. The archons and the ædiles were, in all probability, more difficult in their choice of pieces than the directors of thoſe ſpectacles at preſent. Dramatic authors did not labour in haſte for pitiful wages. Sophocles and Menander were crowned in the aſſemblies of the people, at the Olympic games, and before the eyes of all Greece. Between good taſte and good morals there is more analogy than is generally believed. There is a certain exquiſite ſenſe of propriety which ariſes from a happy diſpoſition, cultivated by a liberal education; and it is of importance that this ſenſe ſhould never be ſuffered to become weak, and that all our inſtitutions, our uſages, and above all our theatres, ſhould tend to ſtrengthen it.

Cit. Langles has publiſhed the three firſt volumes of a collection of Voyages in Aſia, tranſlated from different Oriental and European languages, with a ſhort account of the revolution in Perſia, a memoir on Perſepolis, and hiſtorical notes.

Cit. Schweighauſen, aſſociate of the Inſtitute, who has already given editions of Appian and Polybius, has juſt publiſhed a new one of Epictetus and Cebes enriched with learned notes.

Cit.

Cit. Dupuis has publiſhed an abridgment, in one volume, of his large work on the origin of all the different forms of religious worſhip.

THE ROYAL SOCIETY OF LONDON.

This learned Society held its firſt meeting for the ſeaſon on Thurſday the 8th of November.

An abſtract of a paper (read at a former meeting) entitled "Experiments to determine the Denſity of the Earth, by Henry Cavendiſh, Eſq. F. R. S. and A. S." was read to the meeting. Theſe experiments, which are extremely ingenious and intereſting, are detailed at full length in Part II. of the Tranſactions for the preſent year. They were projected by the late Rev. John Michell, F. R. S. but he did not live to carry them into effect. After his death the apparatus came to the Rev. F. J. H. Wollaſton, Jackſonian Profeſſor at Cambridge, who transferred them to Mr. Cavendiſh. The apparatus contrived for making ſenſible the attraction of ſmall quantities of matter, and which has been improved by Mr. C. is very ſimple: it conſiſts of a wooden arm 6 feet long, ſuſpended by the middle in an horizontal poſition by a ſlender wire 40 inches long; to each extremity is hung a leaden ball about 2 inches in diameter; and the whole is incloſed in a wooden caſe to defend it from the wind.

As no more force is required to turn this balance on its centre, than is neceſſary to twiſt the ſlender ſuſpending wire, the ſmalleſt degree of attraction of a leaden weight or weights, a few (eight) inches in diameter, brought near to the ſmall ſuſpended ball or balls of the balance, will be ſufficient to move it ſenſibly aſide.

To determine from hence the denſity of the earth, all that is neceſſary is to aſcertain what force is required to draw the arm aſide through a given ſpace, and then to have recourſe to calculation.

To prevent any diſturbance from currents that might be produced within the box that contained the balance, by even the difference of temperature that might be occaſioned by heat being communicated by the bodies of the experimenters to one ſide of it more than another, it was ſupported in the middle of a cloſe room; the operators, from adjoining apartments, viewed the operation through holes in the wall by means of teleſcopes; and the apparatus had a ſtrong light thrown upon its two ends (an opening being left at each end of the box for the purpoſe) by means of two lamps, alſo in the adjoining apartments, the rays from which were likewiſe made to paſs through holes formed in the wall.

The two large balls were ſuſpended from a beam near the cieling, which could be moved in an horizontal direction, by means of a ſtring and pulley, ſo as to be brought near to the ſmall balls of the balance, or made to recede again without requiring any perſon to be in the room.

From this deſcription it will be eaſily ſeen that, on the two large balls being brought near to the two ſmall ones, but on oppoſite ſides of each that their forces may not counteract each other, the ſmall ſuſpending wire of the balance muſt be twiſted by the movements of the arms, occaſioned by attraction, which carries the ſmall towards the large balls; and that the wire endeavouring to untwiſt itſelf will again in its turn carry the ſmall balls away from the large ones. Vibrations are thus occaſioned, which would continue a long time before the ſmall balls would ſettle between the firſt point of reſt and the large balls: but it is not neceſſary to wait for this; an ivory ſcale at each end of the balance enables the experimenters, by means of their teleſcopes, to ſee the two extreme diviſions to which the ſmall balls move in their vibrations, and thus to determine the middle point. The time neceſſary for each vibration is alſo noticed.

It would be impoſſible in a ſhort notice to do juſtice to Mr.

Mr. Cavendiſh's ingenious experiments, and the calculations founded on them. Thoſe who feel themſelves intereſted in them will have recourſe to the original account. We ſhall only mention the reſult. By a mean of the experiments the denſity of the earth comes out 5·48 times greater than that of water.

By the experiments made by Dr. Maſkelyne on the attraction of the hill Schehallien, the denſity of the earth was computed to be only $4\frac{1}{2}$ times that of water.

A paper * by Mr. Home was alſo read, containing an account of ſome experiments made in order to aſcertain the cauſe of the light ſeen in the eyes of cats and ſome other animals in the dark. After enumerating the opinions of other philoſophers, he proceeds to ſhew, that when the light was perfectly excluded from the room in which the cat was placed, the eyes were never obſerved to ſhine; whence he concludes that the eye merely collects the light diffuſed through the room. He then relates ſome obſervations on the ſtructure of the optic nerve. Having diſſected the eye of a cat juſt killed, he was ſurpriſed at finding the retina *tranſparent*. This induced him to repeat the experiment not only on cats, but alſo on horſes; and always with the ſame reſult, if the retina was examined immediately after death. But if this examination was delayed an hour or two, it was opake and whitiſh, as deſcribed by anatomiſts. The optic nerve, from the retina to the brain, ſeemed to be compoſed of bundles of fibres not parallel to each other, but alternately interwoven and ſeparated, the interſtices filled with a tranſparent fluid, ſo that a tranſverſe ſection taken near the brain was nearly a circle containing about 40 opake round ſpots; near the eye about 200 of theſe ſpots, and in the middle about a mean between theſe numbers. Theſe obſervations were made with a microſcope magnifying about 23 times.

* The Croonian Lecture.

At their ſecond meeting, on the 15th, a paper by the Rev. Mr. Vince on an unuſual atmoſpherical refraction was read.—One day laſt ſummer, directing his teleſcope to the ſea, he obſerved part of the maſt of a cutter (the hull being below the horizon), and above it an inverted image of the cutter, above which appeared a direct image of the ſame: theſe two images were joined at their hulls; but the point of the maſt of the inverted image appeared to be as far above the top of the real maſt as this laſt was above the horizon. Theſe images appeared and diſappeared very ſuddenly, ſomething like the beams of the aurora borealis, beginning a little above the real object, and darting upwards.—The learned author, after ſuppoſing theſe appearances to be occaſioned by atmoſpheric ſtrata of different denſities, recommends it to philoſophers to inveſtigate this ſubject; and ſuggeſts the utility of ſtationing men provided with teleſcopes at certain places on the coaſt; as by this means he thinks veſſels and other objects might be ſometimes diſcovered long before they would otherwiſe become viſible.

On the 22d a paper, by the Abbé Mann, was read, the ſubſtance of which was to ſhew, that after having travelled ſeveral times over Germany he found very few remains of volcanos. There was very little intereſting matter in this paper, which concluded with an account of ſome remarkable hail-ſtorms, in which ſtones, or pieces of ice, from half an inch diameter to eight pounds weight had fallen.

MISCELLANEOUS.

ASTRONOMY AND GEOGRAPHY.

The aſtronomer Beauchamp, well known by his travels in the eaſt, and formerly grand vicar of Babylon, has rendered a conſiderable ſervice to geography by his new map of the Black Sea; the exact poſition of which he has determined from aſtronomical obſervations. In the year 1781 this active aſtronomer went to Bagdad, where he built an obſervatory. In 1787 he determined the poſition of the ſouthern extremity

extremity of the Caſpian Sea, which before that period had been very uncertain, and alſo the geographical poſition of the cities of Caſbine, Iſpahan, Bagdad, Buſſora or Buſra, and Gella. In the year 1795 he was appointed conſul at Maſcate in Arabia, by the French republic; and ſet out from Paris on the 9th of June, with ſome excellent inſtruments and a chronometer by Berthoud, which he received from General Calon, director of the geographical depôt; but on account of the danger to which French veſſels were then expoſed, in the Mediterranean, he did not arrive at Corfu till the 25th of June 1796; and in the end of October reached Conſtantinople. In the courſe of his voyage through the Grecian Archipelago, he determined the poſition of Patras on the 22d of Auguſt; of Corinth on the 26th; of Naples in Romania on the 28th; of the iſland of Specia on the 1ſt of September; of the iſland of Andros on the 20th; of the iſland of Scio on the 21ſt; of Metelino on the 24th; and of the Cape of the Janiſſaries, in Natolia, on the 27th; &c. Between Corfu and Conſtantinople alone he determined the geographical poſition of twenty-ſix places; and by theſe means rectified the uncertain extent from weſt to eaſt of the Lepantine gulf. By the chronometer he found the longitude of Patras to be 39° 41′ 15″; and that of Corinth 40′ 48′ 15″, conſequently their difference of longitude 1° 7′. At Conſtantinople Citizen Aubert-Dubayet was obliged to negotiate a long time with the Porte before he could obtain permiſſion for Cit. Beauchamp to explore the coaſts of the Black Sea. For the greater ſecurity, he hoped to obtain a *kirlan-kiche*, that is, a ſmall veſſel belonging to the Captain Pacha; but after a great deal of trouble he could procure only a firman. In the year 1780 the Turks ventured to permit the then French ambaſſador, Count Choiſeul-Gouffier, to cauſe obſervations to be made on the coaſts of that ſea; but Achilles Tendu*, the aſtronomer ſent thither for that purpoſe,

* Achilles Tendu was the younger brother of the unfortunate Pierre Marie Tendu, guillotined at Paris on the 28th of December 1793, at the age

purpoſe, was obliged to return, without having accompliſhed the object of his miſſion. This circumſtance was, in part, owing to the jealouſy of the Ruſſians and other European Powers; and beſides, the French had then loſt two friends, men of conſiderable intelligence, viz. the Vizier Halil Pacha, who eſtabliſhed a ſchool for artillery and engineering, and cauſed the beſt French works on theſe ſubjects to be tranſlated into the Turkiſh language, but who was beheaded at Tenedos; and the Vice-Admiral Captain Bey, who poſſeſſed a great many aſtronomical and nautical inſtruments, and who cauſed Cit. de Lalande's abridgement of his aſtronomy to be tranſlated into the Turkiſh language, but who was alſo decapitated in October 1787. Cit. Beauchamp, however, was more fortunate, as he arrived on the 26th of June 1797 at Trebiſonde; and, without any impediment, was enabled to determine the exact poſition of a great many points of the Black Sea. He found the latitude of Sinope to be 42° 2′, inſtead of 41 as it is laid down even in the beſt maps; ſo that the very uncertain breadth of this ſea, between Cape Karadzé and Cape Indgé, which was reckoned to be 62 French leagues, appears to be no more than thirty-ſeven leagues. The longitude of Trebiſonde he found to be 57° 16′ 15″. According to a Turkiſh map of the Black Sea, printed at Conſtantinople in the year of the Hegira 1137, that is 1724 of the Chriſtian æra, there is an error of half a degree of longitude in the poſition of this city; and in the map of the Ruſſian empire, publiſhed in 1776 by Treſcot and Schmid, this error extends to a degree and a half. The calculation of the Jeſuit P. de Bèze is totally falſe, and too great by 7½ degrees. We may, therefore, form ſome idea of the ſtate of geography in Turkey, and of the important ſer-

age of forty-eight. The latter is better known under the name of Lebrun, and who for ſome time held the office of miniſter for foreign affairs. His firſt occupation, however, was that of an aſtronomer; for he aſſiſted Caſſini III. and IV. in the Obſervatory at Paris till the year 1778, and inſtructed in aſtronomy his brother Achilles, who died at Conſtantinople in 1787, at the age of twenty-eight.

vices

vices rendered by Cit. Beauchamp to that useful science. He was accompanied by Charles Hyacinth Receveur, his pupil, who, though only seventeen years of age, was an accurate observer and an able calculator *. On the 4th of September they returned to Constantinople; and on the 20th of October set out for Bagdad. M. Beauchamp, besides the services he rendered to geography and astronomy, transmitted to the National Museum at Paris a great many plants, seeds and insects; and to the literary class of the National Institute a number of unknown ancient Greek inscriptions. He found the variation of the magnetic needle at Constantinople to be 12° 33′; and at Trebisonde 8° 14′.

CONVERSION OF IRON INTO CAST STEEL.

A new method of preparing cast steel has been lately announced in France by Cit. Clouet. His process is as follows: Take small pieces of iron, and place them in layers in a crucible with a mixture of the carbonate of lime. Six parts of the carbonate of lime, that is chalk, marble, limestone, and in general all calcareous substances, and six parts of the earth of pounded Hessian crucibles must be employed for 20 parts of iron. This mixture must be so disposed that, after fusion, the iron may be completely covered by it, so as to be kept from coming into contact with the atmosphere. The mixture is then to be gradually heated, and at last exposed to a heat capable of melting iron. If the fire be well kept up, an hour will generally be found sufficient to convert two pounds of iron into excellent and exceedingly hard steel capable of being forged, an advantage not possessed by steel procured in the common manner.

EXTIRPATION OF THE SMALL-POX.

M. Lenz, Professor at the Institute of Education of Schnepfenthal near Gotha, known by his Travels through Sweden,

* This young man died since at Aleppo, on his way to Bagdad. Edit.

has

has been employed for twelve years in collecting every thing that the phyſicians of different countries have written for or againſt the total extirpation and annihilation of the ſmall-pox by means of police regulations. He propoſes to publiſh a work, in which he will give the reſult of his reſearches on this ſubject, and on the poſſibility of the attempt, and will pay great attention to ſtate with accuracy as many facts as poſſible. In the German Journals he invites all thoſe who have at heart the happineſs of mankind, to point out to him in a preciſe manner, either ſuch facts as have come within their own knowledge, or paſſages in books of travels which contain an account of meaſures that have been taken by the order of governments, or only by individuals, among poliſhed or ſavage nations, at any period, or in any country, by which this terrible contagion has been either removed or entirely checked.

A NEW PROCESS FOR FORMING THE UNGUENTUM HYDRARGYRI.

As the proceſs of combining mercury with hog's lard is well known to be tedious, and to require a conſiderable time, the following method of ſhortening it has been announced by Göttling in his *Taſchen-Buch für Scheidkunſtler* for the year 1798. This proceſs, ſays he, can be ſpeedily performed by the addition of a very ſmall quantity of the flowers of ſulphur. For a mixture of two ounces of hog's lard and ſix drachms of quickſilver, it will be neceſſary to employ only ſix grains of the flowers of ſulphur, and the proceſs will be completed in a few minutes. I do not ſee, adds M. Göttling, that ſo ſmall a quantity of the flowers of ſulphur can hurt the effect of the mixture, and am of opinion that this preſcription may be of conſiderable uſe in pharmacy.

On Mr. CARTWRIGHT's *Invention for rendering the Piſtons of Steam Engines tight by metallic Parts, without Packing or Leather.*

To Mr. TILLOCH.

SIR, Richmond, Nov. 10, 1798.

MY next door neighbour Mr. A. R. having requeſted Mr. Nicholſon* to give his opinion on Mr. Cartwright's new method of packing ſteam engines, and Mr. Nicholſon having, in conſequence, given a much leſs favourable judgment on the merits of the invention than that which appeared in the firſt number of your Magazine, I ſhall be much obliged if you will examine what he has advanced, and ſtate in your next number, whether you think his objections well founded, &c.

Your conſtant reader,

B. S.

No one will withhold from Mr. Nicholſon the tribute of praiſe, to which the numerous ſervices he has rendered and ſtill continues to render to ſcience ſo juſtly entitle him; nor will it any way diminiſh his acknowledged merit, ſhould it be ſhown that the opinion he has delivered on Mr. Cartwright's invention is founded on erroneous principles. Mr. N. has miſtaken the conſtruction of the piſton, deſcribed another, and then endeavoured to ſhow *that triangles will not fit a cylinder*, nor a wafer occupy the diameter of a millſtone. Theſe are not his words; but they are the fair inference from his mode of expreſſion, as we ſhall ſee immediately. But firſt we ſhall briefly ſtate how the piſton is *really* conſtructed. Two metal rings are ground, by means well known to good mechanics, into the cylinder, ſo as *to fit it* as perfectly as art and induſtry can make them—that is, ſo well that no ſteam can paſs between them and the cylinder: their upper and under ſides are alſo ground perfectly flat, and,

* See Mr. Nicholſon's Phil. Journ. vol. ii. p. 264.

though

though not abſolutely neceſſary, for the greater ſecurity two other rings are fitted to the inſide of theſe. On the upper rings is placed a plate of metal, alſo ground perfectly flat, and of ſuch a diameter as almoſt to fit the cylinder: a ſimilar flat plate is placed below the under ones; and the two plates with the rings between are attached to each other by means of the piſton rod that paſſes through them. It is plain then, that ſuppoſing neither the outſide rings nor the cylinder to be able to wear one another, ſuch a piſton would remain ſteam-tight: but as conſtant friction muſt inevitably tend to widen the cylinder and diminiſh the diameter of the rings, the piſton after ſome time would ceaſe to fit, if a contrivance had not been fallen upon to remedy the evil. The rings are each of them cut into three pieces; and, in cutting them, ſuch a portion of the metal is taken away as to leave room to introduce, between two of the pieces, a ſpring in the form of the letter V, the open end of which is placed outwards, almoſt cloſe to the circumference; by which means the two pieces againſt which the two ſides of the ſpring act, are preſſed, *in the direction of the circumference*, againſt the ends of the third piece; ſo that the three pieces are thus kept ſo uniformly in contact with the cylinder, that *the longer the machine is worked the better the rings muſt fit.* To prevent ſteam from paſſing through the cuts in the rings, the ſolid parts of the upper rings are made to fall upon the diviſions and ſprings of the under ones. This is the method contrived by Mr. Cartwright for making his piſton fit; which will be clearly underſtood by looking at Plate I. Vol. I. of this Magazine. If he had left it to "a very moderate portion of mechanical knowledge to ſuggeſt the manner in which the pieces might be made to recede outwards, by means of ſprings," it might perhaps have been propoſed to make ſuch ſprings act from the centre on the circumference: in that caſe, the preſſure of the different parts of each ring could never have been made uniform, and the machine would ſoon have worked itſelf into inequalities, inſtead of working itſelf more true.

If

If the piſton was made of a *larger* or *leſs* diameter than the cylinder, and this difference was endeavoured to be accommodated by means of ſprings, then Mr. N.'s objections to this part would be well founded: in the former caſe the pieces of the rings would only touch the cylinder at their extremities, and in the latter only in their middle—6 points only would be in contact. But if we ſuppoſe the piſton rings of a larger diameter [how came Mr. N. to think of ſuch an abſurdity?]—let us at once ſuppoſe their diameter equal to that of the earth—each portion then would not differ ſenſibly from a ſtraight line. This however is only ſaying, that if Mr. Cartwright attempt to make a triangle fit a cylinder by means of ſprings, he will fail: ſo he would alſo if he ſhould with half an inch endeavour to fill a mile. Mr. C. does not propoſe making any other figure or dimenſion fit his cylinder but its own; and has deviſed a mean by which it muſt, when once fitted, continue ſo.

Mr. Nicholſon is ſtill more unfortunate in his ſecond objection. "When," ſays he, "a great preſſure, ſuch, for example, as the re-action of a column of 100 feet of water, comes to be exerted upon the face of this apparatus, the plates or pieces of theſe rings may be imagined to be confined in a vice. The preſſure of ſuch a column will amount to more than 40 pounds upon every ſquare inch. Whence we may conclude, either that they would not move at all [outward, he means, againſt the cylinder], or that the force of the ſprings muſt be ſuch as greatly to load the work with friction."——If ſuch a re-action could poſſibly take place in Mr. Cartwright's ſteam engine as would *at one and the ſame time* lay a force equal to 40 pounds per ſquare inch *upon both ſides of the piſton*, it would be indeed in a vice; but not ſuch a one as would cauſe friction from the action of the ſprings; for *this vice* would keep the piſton at reſt—it would neither aſcend nor deſcend. But every one acquainted with mechanics muſt know, that, to make a well-conſtructed ſteam engine

gine work, *the preſſure muſt act only on one ſide of the piſton at a time!*

In Mr. C.'s engine, the piſton is not intended to adapt itſelf to *inequalities in the cylinder*; all inequalities are firſt removed. In the working, the piſton aſcends in *vacuo*, by the momentum communicated to the fly-wheel; therefore at that time *there is no preſſure on either the upper or under ſide of the piſton*, and the ſprings are in full action, preſſing, lightly and uniformly, the whole circumference of the rings againſt the cylinder. When the piſton reaches the top of the cylinder it raiſes the ſteam valve; and the ſteam then entering acts *only on the upper ſurface of the piſton*, and forces it to deſcend; but, condenſation having taken place below the piſton, the whole under ſpace is a vacuum, more or leſs perfect, and therefore there can be no re-acting power to perform the office of the vice!

Whether this contrivance will anſwer for pumps and other hydraulic apparatus, is not the preſent enquiry. It will be time enough to give an opinion on that point when we ſee what mode of conſtruction may be uſed in ſuch an application of the invention. A. T.

ERRATUM.

In our laſt Number, page 111, in the Recipe for preparing oxygenated Ointment, for Nitric Acid ℥ij read ℥ij.

THE PHILOSOPHICAL MAGAZINE.

DECEMBER 1798.

I. *Account of a remarkable fiery Meteor ſeen in Gaſcony on the 24th of July* 1790; *by M.* BAUDIN, *Profeſſor of Philoſophy at Pau. With ſome Obſervations on Fire-Balls and Shooting-Stars, by Profeſſor* CHLADNI *at Wittenberg. From* Magazin für das Neueſte aus der Phyſik, *by Profeſſor* VOIGT. *Vol. XI.*

ABOUT half an hour after ten o'clock on Sunday evening, July 24, 1790, as I was walking in the court of the caſtle of Mormes, along with M. de Carrits Barbotan, the atmoſphere being perfectly calm and ſerene, and not a cloud to be ſeen, we found ourſelves ſurrounded, all of a ſudden, by a whitiſh clear light, which obſcured that of the moon, though the latter ſhone with great luſtre, as it wanted only thirty hours of being at the full. On looking upwards we obſerved, almoſt in our zenith, a fire-ball of a larger diameter than the moon. It had behind it a tail, the length of which ſeemed to be equal to about five or ſix times the diameter of the body: at the place where it was connected with the body it had about the ſame breadth, and decreaſed gradually till it ended in a point. The ball and the tail were of a pale white colour; but the point of the latter was

almoſt as red as blood. The direction of this meteor, which proceeded with great velocity, was from ſouth to north.

Scarcely had we looked at it for two ſeconds when it divided itſelf into ſeveral portions of conſiderable ſize, which we ſaw fall in different directions, and almoſt with the ſame appearance as the burſting of a bomb. All theſe different fragments became extinguiſhed in the air, and ſome of them, in falling, aſſumed that blood-red colour which I had obſerved in the point of the tail. It is not improbable that all the reſt may have aſſumed the ſame colour; but I remarked only thoſe which proceeded in a direction towards Mormes, and which were particularly expoſed to my view.

About three, or perhaps two minutes and a half after, I am not certain which, as I was not reflecting upon what might be the conſequences, and therefore did not look at my watch, we heard a dreadful clap of thunder, or rather exploſion as if ſeveral large pieces of ordnance had been fired off together. The concuſſion of the atmoſphere by this ſhock was ſo great that we all thought an earthquake had taken place. The windows ſhook in their frames, and ſome of them, which probably were laid-to and not cloſely ſhut, were thrown open. We were informed next day, that in ſome of the houſes at Houga, a ſmall town about half a mile diſtant from Mormes, the kitchen utenſils were thrown from the ſhelves, ſo that the people concluded there had been an earthquake. But as no movement was obſerved in the ground below our feet, I am inclined to think that all theſe effects were produced merely by the violent concuſſion of the atmoſphere.

We proceeded into the garden, while the noiſe ſtill continued, and appeared to be in a perpendicular direction above us. Some time after, when it had ceaſed, we heard a hollow noiſe, which ſeemed to roll along the chain of the Pyrenees, in echoes, for the diſtance of fifteen miles. It continued about four minutes, becoming gradually more remote, and always weaker; and at the ſame time we perceived a ſtrong ſmell of ſulphur.

While

While we were endeavouring to point out to ſome perſons preſent the place where the meteor had divided itſelf, we obſerved a ſmall whitiſh cloud, which aroſe perhaps from the vapour of it, and which concealed from us the three ſtars of the great bear lying in the middle of thoſe forming the ſemicircle. With ſome difficulty, however, we could at laſt diſtinguiſh theſe ſtars again behind the thin cloud. There aroſe, at the ſame time, a freſh gentle breeze.

From the time that elapſed between the burſting of the ball and the exploſion which followed, I was inclined to think that the meteor was at the height of, at leaſt, ſeven or eight miles, and that it fell four miles to the north of Mormes. The latter part of my conjecture was ſoon confirmed, by an account which we received, that a great many ſtones had fallen from the atmoſphere at Juliac, and in the neighbourhood of Barbotan. One of theſe places lies at the diſtance of about four miles to the north of Mormes, and the other at about the diſtance of five to the north-north-weſt.

M. de Carrits Barbotan, who was at Juliac two days after, confirmed to us the truth of this circumſtance; and it appeared from the accounts of ſeveral intelligent perſons, highly worthy of credit, that the meteor burſt at a little diſtance from Juliac, and that the ſtones which fell were found lying in an almoſt circular ſpace, about two miles in diameter. They were of various ſizes. I have not heard of any houſes being damaged, though ſome of the ſtones fell in courts and gardens. In the neighbouring woods ſome branches were found broken and torn by the falling of the ſtones, which as they deſcended made a ſtrong whiſtling noiſe that many perſons heard. I was told alſo by people of reſpectability, that as the meteor proceeded in its courſe they heard a ruſhing noiſe and ſnapping, like that of electrical ſparks; which appears to me very natural, though theſe were heard neither by myſelf nor M. de C. Barbotan.

Some ſtones were ſeen to fall, which, when found, weighed 18 or 20 pounds, and which had ſunk into the earth from two to three feet. I was told alſo that ſome were found which weighed 50 pounds. M. de C. Barbotan procured one weighing 18 pounds, which he tranſmitted to the Academy of Sciences at Paris. I examined a ſmall ſtone which was brought to me, and found it very heavy in proportion to its ſize: it was black on the outſide; of a greyiſh colour in the inſide, and interſperſed with a number of ſmall ſhining metallic particles. On ſtriking it with a piece of ſteel, it produced a few ſmall dark red ſparks, not very lively. A mineralogiſt, to whom a like piece of ſtone from the ſame meteor was ſhown at Paris, deſcribed it as a kind of grey ſlag mixed with calcareous ſpar, the ſurface of which exhibited vitrified, blackiſh calx of iron. I was told alſo that ſome ſtones were found totally vitrified.

This meteor was ſeen at Bayonne, Auch, Pau, Tarbes, and even at Bourdeaux and Thouloufe. I learned that in the laſt-mentioned place it excited no great attention; which is not ſurpriſing, when we conſider the great diſtance. It appeared there only ſomewhat brighter than thoſe ſhooting ſtars which are ſeen from time to time; and after it burſt, there was heard a hollow report almoſt like a diſtant clap of thunder.

Had it been accurately obſerved at Auch and Pau what ſtars were obſcured by the vapour that aroſe from the burſting of the meteor, the real height of it might have been thence determined with preciſion.

Such, ſays Profeſſor Chladni, is the account given by Baudin of this meteor; the phenomena of which he endeavours to explain from accumulations in the upper parts of the atmoſphere.

According to all the obſervations hitherto made with any accuracy on fire-balls, the height at which they were firſt perceived was always very conſiderable; and by comparing

the

the angles under which they were ſeen from different points, often 19 German miles, and even more; their velocity, for the moſt part, ſeveral miles in a ſecond; and their ſize always very great, often a quarter of a mile and even more in diameter. They were all ſeen to fall moſtly in an oblique direction: not one of them ever procceded upwards. All of them have appeared under the form of a globular maſs, ſometimes a little extended in length and highly luminous; having behind it a tail, which, according to every appearance, was compoſed of flames and ſmoke. All of them burſt after they were ſeen to move through a large ſpace, ſometimes over ſeveral diſtricts, with an exploſion which ſhook every thing around. In every inſtanee where there has been an opportunity of obſerving the fragments that fell after they burſt, and which ſometimes have ſunk to the depth of ſeveral feet into the earth, they were found to conſiſt of ſcorious maſſes which contained iron in a metallic or calcined ſtate, pure, or elſe mixed with different kinds of earth and ſulphur. All the ancient and modern accounts, written partly by naturaliſts and partly by others, are ſo eſſentially ſimilar, that the one ſeems to be only a repetition of the other. This conformity in accounts, the authors of which knew nothing of thoſe given by others, and who could have no intereſt in fabricating ſimilar tales, can ſcarcely have ariſen from accident or fiction, and gives to the related facts, however inexplicable many of them may ſeem, every degree of credibility. In my eſſay on the maſs of iron found by Profeſſor Pallas in Siberia, and others of the like kind, and ſeveral natural phenomena therewith connected *, I have collected the principal obſervations made on fire-balls and the falling of ferruginous ſcorious maſſes obſerved at the ſame time, and have given a kind of explanation, which, however romantic it may ſeem, yet agrees better, in my opinion, with the facts hitherto obſerved than any other, and is contrary to no law of nature hitherto known. Some critics, as

* See the Philoſophical Magazine, Vol. II. p. 1.

well as others, have ridiculed my ſingular hypotheſis, or condemned it altogether; but no one has yet confuted my principles, or given any other explanation that correſponds ſo well with facts. On the other hand, I could mention ſeveral naturaliſts, who, as I know from their own mouths, agree with me in the eſſential parts of my explanation, did I not conſider it improper to bring them thus forward to public notice without their expreſs permiſſion. The ſtrongeſt objection made to my aſſertions is, that ſuch a maſs, from ſuch a prodigious height, would not ſink to the depth of a few feet, but to the centre of the earth. This aſſertion, however, contradicts itſelf; becauſe ſuch a maſs is not ſolid, as may be concluded from the variation of its form ſo often remarked, and the increaſe of its ſize till it at length burſts; but conſiſts of ſoft and elaſtic fluids, which, probably being expanded by the heat, extend to a monſtrous globular form; is then ſupported by the atmoſphere, and loſes the greater part of its gravity. To this may be added, that a ſoft tough maſs, which beſides falls in a very oblique direction, would not in general ſink ſo far into the earth as a ſolid maſs that falls in a perpendicular direction.

The above-deſcribed meteor ſeems, in every point of view, to confirm my method of explanation as much as if it had abſolutely taken place for that purpoſe. This much, at any rate, is proved, that all the phenomena which accompany fire-balls, as well as the falling of maſſes of ferruginous earth and ſulphureous maſſes, obſerved at the ſame time, cannot be explained from accumulations in the upper regions of the atmoſphere, as it can hardly be ſuppoſed that ſuch groſs ſubſtances could float or be diſſolved in ſo rarefied air at a height of 20 German miles, ſo as to be collected and to unite into monſtrous maſſes. As we are acquainted with no power which is able to force ſuch large bodies to ſo conſiderable a height, and then to give them an oblique movement downwards, and ſometimes almoſt horizontal, with a velocity which is equal to that of the planets in their orbits;

and

and as no ſuch maſs has ever been ſeen to aſcend; they appear not to be terreſtrial, but coſmical bodies. Should this not be admitted, it is much to be wiſhed that ſome other perſon might give an explanation conſiſtent with the obſerved facts; and that more attention than has hitherto been beſtowed, were paid to the obſervation of fire-balls and ſhooting ſtars; as for example, that obſerved on the 8th of March 1796 in Luſatia, and in ſome parts of Saxony and Brandenburg.

Shooting ſtars are perhaps meteors of the ſame nature as thoſe teleſcopic ſparks of light obſerved by Mr. Schröter, and may be different from fire-balls only in this, that they move at a much greater diſtance from our earth, and that they do not fall, but only occaſion a tranſient luminous appearance in their paſſage through the upper regions of the atmoſphere.

I ſhall here mention an idea which does not proceed from myſelf, but from a very intelligent aſtronomer, that ſhooting ſtars might be employed to determine the difference of two meridians. Two or more aſtronomers, reſiding at ſome diſtance from each other, might agree to make obſervations on ſhooting ſtars, which appear almoſt at all times, when the weather is clear, in ſome part of the heavens, not with inſtruments, but merely with the naked eye; and to remark, not only the time of their appearance, but alſo their apparent courſe; and from the difference of the times of ſeeing theſe ſhooting ſtars, the difference of the meridians of the places might be determined; and from the difference of their apparent courſes, their real height and real courſe might be diſcovered.

II. *Account of a ſingular Inſtance of Atmoſpheric Refraction. In a Letter from* WILLIAM LATHAM, *Eſq. F.R.S. and A.S. to the Rev.* HENRY WHITFIELD, *D.D. F.R.S. and A.S. From the* Philoſophical Tranſactions of the Royal Society of London, 1798.

DEAR SIR, Haſtings, Auguſt 1, 1797.

ON Wedneſday July 26, about five o'clock in the afternoon, while I was ſitting in my dining-room at this place, which is ſituated upon the Parade, cloſe to the ſea-ſhore, nearly fronting the ſouth, my attention was excited by a number of people running down to the ſea-ſide. Upon enquiring the reaſon, I was informed that the coaſt of France was plainly to be diſtinguiſhed by the naked eye. I immediately went down to the ſhore, and was ſurpriſed to find that, even without the aſſiſtance of a teleſcope, I could very plainly ſee the cliffs on the oppoſite coaſt; which, at the neareſt part, are between 40 and 50 miles diſtant, and are not to be diſcerned, from that low ſituation, by the aid of the beſt glaſſes. They appeared to be only a few miles off, and ſeemed to extend for ſome leagues along the coaſt. I purſued my walk along the ſhore eaſtward, cloſe to the water's edge, converſing with the ſailors and fiſhermen upon the ſubject. They, at firſt, could not be perſuaded of the reality of the appearance; but they ſoon became ſo thoroughly convinced, by the cliffs gradually appearing more elevated, and approaching nearer, as it were, that they pointed out and named to me the different places they had been accuſtomed to viſit; ſuch as the Bay, the Old Head or Man, the Windmill, &c. at Boulogne; St. Vallery, and other places on the coaſt of Picardy; which they afterwards confirmed when they viewed them through their teleſcopes. Their obſervations were, that the places appeared as near as if they were ſailing, at a ſmall diſtance, into the harbours.

Having

Having indulged my curioſity upon the ſhore for near an hour, during which the cliffs appeared to be at ſome times more bright and near, at others more faint and at a greater diſtance, but never out of ſight, I went upon the eaſtern cliff or hill, which is of a very conſiderable height, when a moſt beautiful ſcene preſented itſelf to my view; for I could at once ſee Dengeneſs *, Dover cliffs, and the French coaſt, all along from Calais, Boulogne, &c. to St. Vallery; and, as ſome of the fiſhermen affirmed, as far to the weſtward even as Dieppe. By the teleſcope, the French fiſhing-boats were plainly to be ſeen at anchor; and the different colours of the land upon the heights, together with the buildings, were perfectly diſcernible. This curious phenomenon continued in the higheſt ſplendour till paſt 8 o'clock, (although a black cloud totally obſcured the face of the ſun for ſome time,) when it gradually vaniſhed.

Now, Sir, as I was aſſured, from every inquiry I could poſſibly make, that ſo remarkable an inſtance of atmoſpherical refraction had never been witneſſed by the oldeſt inhabitant of Haſtings, nor by any of the numerous viſitors (it happened to be the day of the great annual fair, called Rock-fair, which always attracts multitudes from the neighbouring places), I thought an account of it, however trifling, would be gratifying to you.

The day was extremely hot (68° at 10 A. M. 76° at 5 P. M.). I had no barometer with me, but ſuppoſe the mercury muſt have been high, as that and the three preceding days were remarkably fine and clear. To the beſt of my recollection it was high water at Haſtings about two o'clock P. M. Not a breath of wind was ſtirring the whole

* The cape of land called Dengeneſs, which extends nearly two miles into the ſea, and is about ſixteen miles diſtant from Haſtings, in a right line, appeared as if quite cloſe to it; as did the fiſhing-boats and other veſſels which were ſailing between the two places: they were likewiſe magnified to a great degree. L.

of

of the day; but the ſmall pennons at the maſt-heads of the fiſhing-boats in the harbour were in the morning at all points of the compaſs.

I was a few days afterwards at Winchelſea, and at ſeveral places along the coaſt; where I was informed the above phenomenon had been equally viſible.

III. *Deſcription of the* Equus Hemionus *or* Dſhiggetai *of the Eaſtern Deſerts of Middle Aſia. By Profeſſor* PALLAS. *From* Neue Nordiſche Beytrage, *Vol. II.*

[Concluded from page 121.]

THE covering ſeaſon of the dſhiggetai falls towards the middle or end of Auguſt; and about this period Meſſerſchmidt obſerved in a female, which he diſſected, a fœtus not larger than a mouſe. They caſt their young in the ſpring, and generally have one foal at a time. When the young attain to the age of three, they are equal in ſize to the old ones, and are fit for procreation, as I was aſſured by the inhabitants of thoſe countries, who often had an opportunity of killing and examining theſe animals.

The males when they fight bite each other, like the common wild horſes. The dſhiggetai fights alſo with its heels, as was obſerved in a foal which had been caught, and which, a few years before my arrival in Dauuria, was kept there by one of the Coſſacks; but it continued alive only a ſhort time. The hair of the dſhiggetai in winter is almoſt of a pale iſabella yellow colour, about an inch and a half long; a little rough on the back, as was the caſe in the foal of a wild horſe, which I have already deſcribed *, and equally ſoft and tender. Skins covered with winter hair of this kind I have found among the Tunguſians. The female, however, which I procured in Dauuria, at the end of the month

* See *Pallas Reiſe*, vol. iii. p. 509, Plate VIII.

of

of May, had at that period loſt almoſt all its winter hair, though the Tunguſian horſes ſtill retained theirs. The ſummer hair of this female was uncommonly ſmooth and pretty; and I never found it much longer on the ſkins of animals ſhot far later in the ſeaſon. The colour eaſily becomes paler; and on the ſtuffed hide of the female, which I have deſcribed, and which is preſerved in the cabinet of natural hiſtory at Peterſburgh, the ſummer hair, partly by the preparation the hide has gone through, and partly by the effects of the ſun, has been totally bleached to a dull iſabella colour, which appears lighter than the common winter hair.

The above female was about three years old, and was ſhot on the 26th of May O. S. 1772, in the neighbourhood of Tareinoor, in the country of the Tunguſians. As the ſpring heats had then commenced with great violence in thoſe diſtricts, the animal, at Kuluſſutai, where it was kept for me, had already begun to putrefy on the 28th, and expoſed me to a very diſagreeable piece of labour, eſpecially in a barren region, where I was obliged to diſſect it without any aſſiſtance. The figure (Plate V.) done from a drawing made before the diſſection, is ſo accurate, that I was never better ſatisfied with any repreſentation of an animal. The dſhiggetai cannot, therefore, be any longer conſidered among the doubtful animals, and its deſcription is as follows:

In ſize and figure it has a great reſemblance to a middle-ſized mule; but it excels the mule in the beauty and delicacy of its make. The head is larger than that of a horſe, and more compreſſed at the ſides; the forehead is perfectly flat, and runs downwards with a ſmall angle towards the muzzle. The ſides of the head are alſo flat, eſpecially where the broad part of the jaw-bones lie, and where the head has the greateſt thickneſs. On the other hand, the throat, between the broad parts of the jaw-bones, is hollow.

The lips, particularly the upper one, are thick and flabby; thinly covered with hair, and black on the edges, which are beſet

beſet with ſtiff grey briſtles, crooked and bent downwards. The corners of the mouth are covered with ſoft hair on the inſide; and the inner ſide of the cheeks is covered with fine blackiſh glands. The number of the teeth amounts to thirty-four; ſo that this animal has two leſs than the horſe. The fore-teeth are ſix; the four middlemoſt of which, in the above-mentioned female, had not yet been changed; they were of a conical form marked with a ſmall furrow; and the ſide ones were obliquely blunted, and preſſed towards thoſe in the middle. The ſituation of the fore-teeth is the ſame as thoſe of the horſe, ſtanding perpendicular in the upper, and obliquely in the under jaw. I found the eye-teeth neither in this female, nor in the ſkull of a ſtallion, which had been ſhot a little before, and brought to me by the Tunguſians; but I obſerved in the upper jaw of the former, at a mean diſtance between the fore and jaw teeth, a trace as of a ſocket for a tooth. The jaw-teeth were like thoſe of the horſe; but on each ſide above, as well as below, there were only three fully grown, beyond which the fourth was juſt beginning to appear. I found a fifth in the before-mentioned ſkull concealed within the ſocket. Of the latter, Meſſerſchmidt, ſpeaking of the ſkull of a ſtallion, makes no mention. In the palate I counted ſeventeen broad wrinkles, of which, thoſe before were flatter, and thoſe behind higher and broader.

The noſtrils, as in the horſe, are wide, blackiſh on the inſide and edges; but under the noſtril, on each ſide of the cartilage, there is an excreſcence like a large round wart, which is to be found neither in the horſe nor the aſs. The muzzle is interſperſed with long blackiſh briſtles, the longeſt of which, about two inches in length, are found on the under lip, and on the outſide of the noſtrils.

The eyes are of a moderate ſize, and, with their longeſt diameter, ſtand in the head obliquely. The edges of the eyelids and a triangular ſpot at the corner of the eye are bald and blackiſh; and the upper eyelid only is furniſhed with

with a row of black eyelaſhes, ſeven lines in length, which are at a little diſtance from the edge of the eyelid, and do not reach entirely to the corners. Below the foremoſt corner, however, of the eye, in the neighbourhood of the *os jugalis*, there are ſeveral ſcattered black hairs, two of which, more than two inches in length, lie entirely flat. In the corner of the eye lies a thick white fold of ſkin, which is capable of being extended, ſeven lines in breadth, as far as the iris, and in the middle of its edge has a blackiſh moon-formed ſpot. The white of the eye, next to the tranſparent cornea, is browniſh; the iris is darkiſh grey, and, as it were, radiated with folds; the pupil is elongated, ſo that its longeſt diameter runs through the corner of the eye, and, like the eye itſelf, ſtands obliquely.

The ears are much larger than thoſe of the horſe, though much more in proportion to the ſize than thoſe of the aſs; ſharp-pointed, erect, internally of the colour of the body; but the inſide, at the top, and a ſtripe on the edges downwards, are browniſh black. On the inſide they are covered with curled whitiſh hair, which riſes up from the circumference. Three raiſed ſtripes run along the inſide of the ear lengthwiſe, which are thinly covered with hair of the like kind projecting from each other.

The neck is ſmaller and rounder than that of the horſe, and even than that of thoſe called ſtag-necked. The mane proceeds from the back part of the ſkull to the ſhoulders, of an equal height, and is as ſoft and as upright as in young foals. It is of a blackiſh colour with yellowiſh grey points, and about three quarters of an inch in length. Inſtead of the tuft before, the whole ſpace between the ears and the orbits of the eyes is thick ſet with ſoft hair, of the ſame colour as the mane, and at moſt one inch three lines in length.

The body is pretty long, more compreſſed on the ſides than in the horſe, contracted towards the lower part of the breaſt, particularly before, into a conical form, and ſtrongly built.

built. The rump is pretty ſtraight and ſomewhat ſharp, as in the aſs. As the body was ſwelled up, the ſpine was become bent into the form of a bow.

The animal ſtands high on its limbs, which are delicate, though its muſcles are ſtrong; but the ſhoulders, thighs and legs are ſomewhat meagre, like thoſe of ſlight-formed mules. The fore legs, on the inſide, have a longiſh, round, bald, blackiſh ſcar, covered with a tender, ſomewhat hard and brittle ſkin, which, at ſome times of the year, may perhaps be covered by a corneous ſubſtance. In the common aſs this ſcar is more of a round form. It was in length two inches ſeven lines, and in breadth one inch ſix lines. On the hind legs no traces of ſuch a ſcar were to be ſeen. The lower part of the leg is perfectly ſmooth, ornamented behind with a conſiderable tuft of ſhort hair: on the fore-ſide inwards there is a black ſpot, and outwards a faint mark near the before-mentioned tuft. On the hind feet there are two ſmall faint ſpots above it. The hoofs are very hard, dry, of a black colour, ſmall, and almoſt like a half cone; roundiſh above, and very hollow on the ſole. The edges of all the hoofs exhibited traces of cracks which had grown up again.

The tail was almoſt like that of a cow; the trunk is thin, moderately long, perfectly round, quite bald from the rump to the middle; but the other part is covered with ſhort briſtles of the ſame colour as the ſides of the body. It is buſhy at the end, where it forms a black tuft, about nine inches in length.

I found the winter hair on the ſkin already mentioned to be two inches long, tolerably thick, as ſoft as camel hair, and of an iſabella grey colour outwardly, but of a pale iron-grey towards the roots. The ſummer hair on the female, which I have deſcribed, was ſcarcely three quarters of a line in length, here and there diſpoſed into circular arrangements (*vortices*) and ſhadings, and on the back never turned forwards as in the zebra. A ſhading of the hair runs lengthwiſe

wiſe along the face, and two others above the eyes. The vortices I obſerved firſt on each ſide of the mane immediately behind the ears. There were two alſo above each other under the neck; two on the ſides of the neck oppoſite the ſhoulders; a larger one on each joint of the ſhoulder; another large one on each ſide of the breaſt behind the ſhoulders, and alſo a cruciform ſhading of the hair. Several more of theſe hair vortices could be obſerved on different parts of the body.

The colour of the muzzle is whitiſh; the reſt of the body inclines a little more to yellow; the neck is of a fallow colour; the rump from the back to the ſides of an ochre yellow; the ſides fallow, and the limbs paler. The hind part of the fore legs, and the inner part of the hind legs, together with the lower part of the rump and the hinder edge of the thigh, are whitiſh. Where the mane ceaſes, there begins a blackiſh brown ſtripe, which runs along the ridge of the back to the buſhy part of the tail. The udder is blackiſh and perfectly bald, with two ſtrong ſhort teats. The meaſurement of the principal parts of this animal is as follows: The length from the ſkull between the ears to the root of the tail 5 feet 1 inch 3 lines; length of the head 1 foot 8 inches 6 lines; height of the animal at the ſhoulders before 3 feet 9 inches 9 lines: height behind 4 feet 3 inches 6 lines; length of the ears 7 inches 2 lines; length of the tail without the bruſh at the end 1 foot 4 inches 1 line; length of the bruſh 8 inches 2 lines. The weight of the whole animal amounted to 560 pounds apothecary's weight.

Every thing conſidered, the dſhiggetai is diſtinguiſhed particularly by the head, which has a mean proportion between that of the horſe and the aſs; and, in that reſpect, as well as by the ears and the tail, it approaches very near to the zebra. In the reſt of the body and the thighs it is more like the aſs; and in the limbs it has a greater ſimilarity to the mule and the horſe. The tail is more naked than that of the aſs, and nearly like that of the cow. Its

colour

colour and hair vortices are peculiar to itſelf. The ſtripe on the back, however, is like that of the female onager, and of many horſes, without any croſs ſtripes. The tail excepted, it has, in general, the greateſt likeneſs to the mule. But, from the above circumſtances, it clearly appears that the dſhiggetai is an animal of a diſtinct genus, and as much peculiar to Aſia as the zebra and quagga are to Africa; whereas the aſs, and perhaps the horſe, in a wild ſtate, belong to both theſe quarters of the world in common.

IV. *Experiments on, and the Manner of diſtinguiſhing, ſeveral Diſeaſes by the Urine. By Mr.* CRUICKSHANK. *From* Caſes of the Diabetes, &c. *Second Edition.*

THE urine is a fluid ſeparated from the blood by the kidneys, containing ſeveral neutral ſalts, with more or leſs animal extractive matter. The relative proportions of theſe ſubſtances are found to vary ſo much in the ſame perſon, both in health and diſeaſe, that it is extremely difficult to fix on what may be conſidered as a ſtandard for healthy urine. The ſpecific gravity of this fluid reaches from 1005 to 1033, that of diſtilled water being 1000: by expoſure to the air it ſoon runs into the putrefactive ſtate, accompanied with the production of much ammonia, although when firſt voided it always contains an exceſs of phoſphoric acid, and reddens litmus: in conſequence of this exceſs, it holds in ſolution more or leſs phoſphat of lime, which may be readily thrown down by a fixed alkali, or even pure ammonia. By evaporation, 36 oz. yield a reſiduum varying from 1 oz. to 1½: this conſiſts of the muriats of potaſh and ſoda, the phoſphats of ſoda, lime, and ammonia, the phoſphoric and lithic acids, with animal extractive matter. Their relative proportions in a healthy ſtate may be nearly as follows:

Oz.

	oz.	dr.	gr.
Muriatic ſalts - - - -	0	1	0
Phoſphoric ſalts - - -	0	3	50
Lithic acid and phoſphat of lime, with exceſs of acid - - - -	0	6	25
Animal extractive matter - -	0	3	40

The neutral ſalts, when purified by cryſtalliſation, are generally very fuſible; but this circumſtance varies much, according to the greater or leſs proportion of phoſphoric ſalts, more particularly the phoſphat of ammonia, on which their fuſibility in a great meaſure depends: inſtead of melting, this ſaline matter ſometimes decrepitates when ſuddenly heated, owing to an exceſs of the muriatic ſalts: of theſe laſt it may be obſerved, that the muriat of potaſh is in general the moſt prevalent, and is eaſily diſtinguiſhed from the muriat of ſoda, by its cryſtalliſing in ſome degree by cooling, and by its affording cream of tartar on the addition of acid of tartar.

In recent urine the ammonial ſalts bear a very ſmall proportion; but when it has become ſtale or putrid, they are much more abundant. The phoſphat of ammonia is the principal, although we have likewiſe met with the muriat of ammonia.

(A.) The lithic acid and phoſphat of lime are generally depoſited, at leaſt in a great meaſure, after the urine has become cold and ſtood for ſome time: the quantity of the firſt varies exceedingly, but that of the laſt we have found, for the moſt part, nearly the ſame, the proportion being about 1 grain to 2 ounces. Theſe ſubſtances may be eaſily diſtinguiſhed, by diſſolving them in twice their weight of nitrous acid, diluted with a little water, and evaporating to dryneſs; the dry maſs when hot will aſſume a beautiful deep roſe or crimſon colour, when the lithic acid is preſent, but will continue white if heated even to redneſs, or have only a ſlight greeniſh tinge when there is nothing but phoſphat of lime. Their relative proportions may be aſcertained by expoſing the mixed maſs for ſome time to a red heat in a cru-

cible; in which caſe, the lithic acid will either burn out or evaporate, leaving the phoſphat in a pure ſtate.

The quantity of extractive matter is more variable than that of any other ſubſtance—hyſterical or crude urine (as it has been called) containing hardly any, while that of concoction abounds with it. This matter yields, by diſtillation, water containing the carbonat of ammonia; this carbonat in a concrete ſtate; a fœtid empyreumatic oil; a little phoſphorus; and laſtly, a reſiduum of animal coal difficult to incinerate: in ſhort, nothing more than the uſual products from animal ſubſtances. If to 1 oz. of this extract be added 1 oz. of the concentrated nitrous acid, diluted with an equal quantity of water, a violent effervescence, accompanied with heat and the diſengagement of nitrous gas, will take place: when the action has ceaſed, and the liquor become cold, a number of ſhining ſcales, or cryſtals, reſembling the acid of borax, will be depoſited, which, when well dried on blotting-paper, will be found to weigh from 5 to 7 dr. or ſometimes more, the proportion varying according to the quality of the extract and the method of conducting the proceſs. The figure of theſe ſcales appears to be that of flat rhomboids; they have a ſmooth greaſy feel when preſſed between the fingers; are ſoluble in much greater quantity in hot than cold water, and alſo in ſome degree in alcohol: although repeatedly waſhed with this fluid, and dried on blotting-paper, they ſtill retain acid properties, and ſtrongly redden the ſyrup of violets; they are readily taken up by the ſulphuric and muriatic acids without commotion; but with the nitrous acid they produce a kind of effervescence, and form very ſoluble neutral ſalts, whoſe properties have not been ſufficiently examined. Their ſolution in water does not precipitate lime-water, nor the muriats of lime or barytes, nor the nitrats of ſilver or mercury, in any ſenſible degree, nor has it any effect upon the ſulphat of iron or acetite of lead; they do not, therefore, contain phoſphoric or oxalic acid: when thrown upon a red hot iron, they melt and evaporate in white

ſmoke,

ſmoke, leaving a very ſmall quantity of a charry reſiduum, by no means difficult to incinerate; but, when expoſed to an intenſe heat, they burn with a reddiſh flame and a kind of detonation ſomewhat ſimilar to nitrat of ammonia.

From theſe experiments it would appear that this ſubſtance is an animal acid hitherto unknown, and whoſe baſis exiſts in this extractive matter.

We ſhall now briefly mention the effects produced by certain re-agents on the urine, when in a healthy ſtate, and likewiſe when altered by ſome morbid diſpoſitions of the body.

Pure ammonia and the fixed alkalies, dropped into healthy recent urine, produce a ſlight cloud, which, on examination, will be found to conſiſt principally of phoſphat of lime; about 2 gr. may be obtained in this way from 4 oz. of urine. Lime-water likewiſe throws down a precipitate which is much more copious, for reaſons that muſt be obvious.

Nitrous acid, added to healthy urine, produces a ſlight efferveſcence, and gives it more or leſs of a reddiſh colour, but produces no precipitation. In ſome diſeaſes, however, particularly general dropſy or anaſarca, this re-agent, when dropped into the urine, produces a milkineſs, and in ſome inſtances a coagulation, ſimilar to what would take place if added to the ſerum of the blood. When bile is mixed with this fluid, as in jaundice, the acid renders it green: a ſimilar effect is produced by the muriatic, and alſo in ſome degree by the ſulphuric acid; for this purpoſe, however, the muriatic is to be preferred.

The principle of tan, or infuſion of oak-bark, detects animal mucilage or jelly, and the quantity of coagulum thrown down will in general bear a certain proportion to the extractive matter. Four oz. of healthy urine afforded in this way a precipitate of about 4 gr.*

* *Seguin* remarks, that in every caſe where the action of the ſtomach and organs of digeſtion is much impaired, this infuſion produces a copious precipitate. C.

The corroſive muriat of mercury is a very uſeful re-agent, as it has no immediate effect upon recent healthy urine; but in every caſe of increaſed action of veſſels, more particularly of the inflammable kind, a greater or leſs milkineſs, and a whitiſh precipitate, is inſtantly produced; it likewiſe in ſome degree coagulates dropſical urine.—Effects ſomewhat ſimilar, although not ſo ſtriking, are produced by alum.

The muriat of barytes detects the phoſphoric ſalts. Four oz. of healthy urine yield with this re-agent a precipitate of 13 gr. equivalent to about 24 or 25 gr. of microcoſmic ſalt, conſiſting of ſuch a mixture of the phoſphats of ſoda and ammonia as are uſually met with in the urine. This ſubſtance will therefore ſhew when theſe ſalts are deficient, or in too great a quantity. The ſulphat of iron has a ſimilar effect, but is not ſo certain, as any little exceſs of acid might prevent the ſeparation of the phoſphat of iron.

The nitrats of mercury and ſilver are decompoſed by the phoſphoric and muriatic ſalts, and likewiſe, in ſome degree, by the extractive matter; they can, therefore, be but of little uſe as re-agents.

The acetite of lead is decompoſed by the muriatic and phoſphoric ſalts: with the laſt it forms an inſoluble precipitate; but the muriat of lead may be diſſolved by 18 or 20 times its weight of hot water. This circumſtance affords a ready method of determining their relative proportions; for, if we precipitate a given quantity of urine by this ſubſtance, dry the precipitate, and then digeſt it in 20 times its weight of diſtilled water, what remains muſt be phoſphat of lead, and the quantity taken up will indicate the proportion of muriat of lead. Four oz. of healthy urine treated in this way yield a precipitate of 31 gr.; this being digeſted in diſtilled water, 7 gr. were taken up, the remaining 24 gr. were found to be phoſphat of lead, equivalent to 23 or 24 gr. of microcoſmic ſalt, as was determined directly by experiment.

It

It muſt be obſerved, that the urine employed in theſe trials ſhould be recent, and not kept for any length of time, as it is well known that ammonia is ſoon produced, which would render the reſults extremely uncertain. It was thought proper to give the quantity of precipitate thrown down by the principal re-agents, when added to healthy urine; for by this means we ſhall be better enabled to detect any remarkable deviation from the healthy ſtandard. The quantity of four ounces was conceived to be the moſt convenient, as in many caſes we might not be able to procure more, and a leſs quantity could only afford uncertain reſults.

Upon the whole, therefore, we would obſerve, that the proportion of extractive matter may, in ſome meaſure, be determined by an infuſion of oak bark, or rather galls; the quantity of phoſphoric ſalts by the muriat of barytes, or acetite of lead; that of the muriatic ſalts by the latter ſubſtance; the proportion of phoſphat of lime by pure ammonia, or any of the alkalies, and the lithic acid by the proceſs already deſcribed. (See A.)

In morbid ſtates of the urine, the coagulable part of the ſerum is detected by the nitrous acid, and even by heat; bile by the nitrous or muriatic acids; and the condition of urine accompanying rheumatiſm and other inflammatory complaints by the corroſive muriat of mercury, and ſometimes by alum. With regard to ſugar which is met with in diabetes, the different methods of detecting it have been already fully explained*: we would only further propoſe, when

* In confirmed diabetes the urine is ſweet to the taſte. In one caſe "Mr. Cruickſhank took 36 oz. troy weight of urine, and it yielded by evaporation 3 oz. 1 dr. of ſaccharine extract, of the appearance of molaſſes, but thicker, having nearly the conſiſtence of wax, and ſomewhat tenacious. If the whole of the day's urine had been evaporated, it would (at the ſame rate) have yielded about 29 oz. an aſtoniſhing quantity to be formed and ſeparated daily from the ſyſtem. By ſtanding in the air it became moiſt,

when the quantity of ſaccharine matter is but ſmall, that the extract obtained by careful evaporation ſhould be treated with twice its weight of nitrous acid, and the whole reduced by evaporation to a very ſmall compaſs: when this has become cold, the cryſtals formed ſhould be carefully examined. If we perceive nothing but ſmall cubes or rhomboids, we may be certain that no oxalic acid has been produced; but if, along with theſe, we ſhould obſerve ſlender needles or priſms, theſe ſhould be carefully ſeparated, dried on blotting-paper, and thrown into lime water: if a precipitate is formed, it muſt either be from phoſphoric or oxalic

and of nearly the conſiſtence, ſmell, and appearance of treacle. Treating ſome of this extract with the nitrous acid, he procured the ſaccharine or oxalic acid; and with a ſmaller portion of the acid it produced a ſubſtance, which, in reſemblance and ſmell, could not be diſtinguiſhed from honey." In another caſe 36 oz. of diabetic urine yielded 3 oz. 1 dr. 10 gr. of reſiduum, apparently more ſaccharine than the above, but having the other reſemblances. Treating it with the nitrous acid it gave the ſame reſult. A quantity of the ſame reſiduum was expoſed to heat in a retort: the firſt product was evidently acid, but on the addition of potaſh the ſmell of ammonia could be perceived—the laſt matter was manifeſtly alkaline, and mixed with a little empyreumatic oil. The ammonia did not come over in a diſengaged form until the bottom of the retort became red hot, and the quantity on the whole was very ſmall.

"In order to aſcertain how far the ſaccharine matter produced in diabetes reſembled that ſecreted in the breaſts of animals, the following experiments were made by Mr. Cruickſhank:

"Two drachms of cryſtalliſed ſugar of milk were treated with 12 dr. of the concentrated nitrous acid, diluted with one oz. of water: after proper evaporation the liquor was ſuffered to cool, when a maſs of cryſtals, mixed with a white powder, was depoſited: theſe being ſeparated and dried on blotting paper, amounted to 58 gr.; about 6 dr. of hot diſtilled water were then added, upon which the cryſtals, conſiſting of oxalic acid, were diſſolved, and the white powder fell to the bottom. The clear liquor being decanted, the remainder was thrown into a filter, and by this means the powder obtained in a ſeparate ſtate, which when dried on blotting-paper amounted to 10 grains. This was found to have all the properties of ſaccholactic acid, as deſcribed by Scheele.

"Two

oxalic acid, and from which, may in a great meaſure be determined by the appearance of the precipitate: if this has the reſemblance of flocks, and ſubſides ſlowly, the phoſphoric acid is the precipitant; but if it has a powdery form, and ſubſides quickly, it muſt be produced by oxalic acid. To this mode we know it may be objected, that all animal ſubſtances yield more or leſs oxalic acid: but this is by no means true to the extent generally ſuppoſed; for there are ſeveral animal ſubſtances, and the extractive matter of healthy urine is one, from which we have never been able to procure any ſenſible quantity of this acid. Of the different parts of the blood, the coagulating lymph is the only one which yields it in any remarkable proportion; and we

"Two drachms of very ſweet extract from diabetic urine were taken, and treated at the ſame time with an equal quantity of nitrous acid, and under exactly ſimilar circumſtances. The liquor in this caſe, when properly evaporated, depoſited a quantity of very tranſparent cryſtals, but no white powder: theſe, when properly dried on blotting-paper, weighed 40 gr. and, on the addition of 6 dr. of hot diſtilled water, were entirely diſſolved.—The ſame experiment was repeated with the ſweet extractive matter from the urine of two other patients; the oxalic acid was procured, but not the leaſt trace of ſaccholactic acid.

"Half an ounce of honey was treated with 3 oz. of nitrous acid in the uſual manner, and it yielded about 2 dr. of pure oxalic acid, but no ſaccholactic. Honey therefore differs eſſentially from the ſugar of milk; and, indeed, from moſt of its obvious properties, would appear to be merely a modification of common ſugar.

"In the courſe of theſe experiments I found that 2 dr. of pretty dry and ſweet diabetic extract yielded, with 12 dr. of the nitrous acid, from 40 to 50 grains of cryſtalliſed oxalic acid, being nearly the proportion which may be procured from an equal quantity of common ſugar, if we make allowance for the water and ſaline ſubſtances. Upon the whole, therefore, it would appear that this extractive matter does not contain the baſis of the ſaccholactic acid—a circumſtance which ſufficiently diſtinguiſhes it from the ſaccharine part of the milk of animals; and ſeems to ſhew that it is nothing more than vegetable ſugar, if we may be allowed the expreſſion, mixed with a greater or leſs proportion of animal mucilage." C.

believe that the coagulable part of the ſerum, when perfectly pure, does not afford it at all. We ſhall only further remark, that the method by fermentation, ſo much relied upon by ſome, is by no means correct.

From what has been delivered it muſt be evident, that an attentive examination of the urine may lead to uſeful concluſions in ſeveral diſeaſes.

In dropſy, the general diſeaſe may readily be diſtinguiſhed from that depending on morbid viſcera, by attending to the effects produced on this fluid by nitrous acid and the corroſive muriat of mercury. In three caſes which we have lately met with, the urine coagulated not only on the addition of nitrous acid, but likewiſe by heat; and, in one of them, which proved fatal in ſix weeks, the urine appeared to differ but little from the ſerum of the blood, ſo remarkable was the coagulation produced by heat and acids. This coagulable ſtate, however, muſt not be confounded with that produced by cantharides; for we have ſeen ſome caſes of violent ſtrangury from this cauſe, where the urine had the appearance of hydatids *.

In the dropſy proceeding from diſeaſed liver and other morbid viſcera, the urine does not coagulate either by nitrous acid or heat: it is uſually ſmall in quantity, high coloured, and depoſits, after ſtanding, a conſiderable quantity of pink-coloured ſediment. This peculiar ſediment we conſider as, in ſome meaſure, characteriſtic of diſeaſed or rather ſcirrhous liver. On examination we found that it conſiſted of a phoſphat of lime, ſome animal matter, to which its red colour was probably owing, and a little lithic acid; this laſt, however, was in very ſmall quantity.

In inflammatory affections, particularly thoſe of the cheſt and acute rheumatiſm, the urine, during the active ſtate of the diſeaſe, always affords an immediate precipitate with the corroſive muriat of mercury or alum, and ſome-

* The infuſion of galls likewiſe coagulates this ſpecies of urine.

times

times with the nitrous acid, when the diſeaſe takes a favourable turn, this effect will in a great meaſure ceaſe, and the lateritious ſediment make its appearance.

In fevers, particularly ſuch as are accompanied with a ſtrong action of veſſels, ſimilar phenomena may be obſerved; but in this caſe the lateritious ſediment, which never fails to make its appearance at the criſis or abatement of the diſeaſe, is much more remarkable and conſtant; and the nitrous acid, when added to the urine before the depoſition takes place, gives it a pretty deep red tinge.

In gout, too, the termination of the paroxyſms is moſt perfectly indicated by a copious lateritious ſediment; and when this ſuddenly diſappears, and the urine at the ſame time affords a precipitate with the muriat of mercury, a freſh attack or relapſe may be expected. We have generally found this ſediment to be compoſed of lithic acid, phoſphat of lime, and ſome peculiar animal matter but little ſoluble in water: it has by ſome been ſuppoſed to conſiſt entirely of lithic acid; but this for the moſt part conſtitutes the ſmalleſt part.

In jaundice, a very ſmall quantity of bile may be detected in the urine by the nitrous or muriatic acids, but more particularly the latter. When, therefore, the urine ceaſes to become green on the addition of theſe acids, we may infer that the obſtruction to its paſſage into the duodenum is removed, and that, conſequently, the yellowneſs of the ſkin, &c. will ſoon diſappear.

The ſpaſmodic affections that take place in many nervous complaints are immediately indicated by the urine, which upon the approach of theſe will moſt generally be found tranſparent, large in quantity, and abounding in ſaline, but containing hardly any extractive, matter. Similar appearances have been obſerved to precede delirium in fevers. This ſtate may be diſcovered by infuſion of oak bark, with which it will hardly give any precipitate, and by the acetite of lead, which will produce a very copious one.

In ſcurvy, and ſome other putrid diſeaſes, as they have been called, we make no doubt the appearances and properties of this fluid are ſufficiently remarkable, and we would recommend it to be examined by thoſe who may have opportunities.

Can internal ulceration and ſuppuration be detected in this way? In ſome diſeaſes of the abdomen, accompanied with tumefaction, the urine has been met with of a white colour, as if mixed with pus; and in a few inſtances, when this change has taken place, the enlargement has ſuddenly ſubſided.—Was this colour owing to pus? and, if ſo, was the pus taken up by the abſorbents, or was it carried off by the kidneys, in conſequence of ſome direct morbid communication? In children ſubject to worms, the urine has likewiſe been obſerved to have at times a white colour; and this has been ſuppoſed to proceed ſomehow from the chyle. This peculiar appearance is not to be confounded with what takes place in local affections of the kidneys and bladder, where it alſo occurs, but is ſufficiently underſtood. We have had no opportunity of examining this kind of urine, and cannot, therefore, give any ſatisfactory account of it.

We have thrown out theſe imperfect hints, merely with a view to induce others to pay ſome attention to a ſubject which has of late been much neglected, but which, in our opinion, is capable of affording great aſſiſtance in the inveſtigation and cure of many diſeaſes.

V. *On the faſcinating Power of the Rattle-Snake, with ſome Remarks on Dr.* BARTON's *Memoir on that Subject* *. *By Profeſſor* BLUMENBACH. *From Profeſſor* VOIGT's Magazin für den neueſten zuſtand der Naturkunde. *Part II.* 1798.

IT has been aſſerted of many animals, both warm and cold-blooded, and particularly of different ſnakes, poiſonous as well as harmleſs, in the old † and new world ‡, that even when at ſome diſtance from other animals, eſpecially thoſe which ſerve them as food, they have ſuch a faſcinating power over them, that they are forced to approach them juſt in the ſame manner as if attracted by them.

The rattle-ſnake has been particularly celebrated on account of this property, which has been denominated its faſcinating power; and naturaliſts have endeavoured to explain it on the following principles. Many ſuppoſe that the ſmall birds, ſquirrels, &c. which have been ſeen to fall from the branches of trees, as it were ſpontaneouſly, into the mouth of the rattle-ſnake, muſt have been previouſly bitten by the ſnake; and that, weakened by the activity of the poiſon, they were incapable either of flying away or of remaining longer on the tree. Some ſuppoſe that the rattle-ſnake, under cer-

* A Memoir concerning the faſcinating Faculty which has been aſcribed to the Rattle-Snake and other American Serpents. By Benjamin Smith Barton, M.D. and Profeſſor of Natural Hiſtory and Botany in the Univerſity of Penſylvania. Philadelphia, 1796. 70 pages 8vo.

† M. le Vaillant ſays, in his New Travels into the interior Parts of Africa, that the faſcinating power of many ſerpents is generally believed by the Hottentots, as well as by many of the Negroes and Moors. B.

‡ Dr. Barton ſays, page 19, that he never found any traces of this aſſertion among the Indians of South America. I however remember to have read of a like idea in the account of many voyages to that quarter of the world. Thus Dobriſhoffer, for inſtance, aſſerts in his Hiſtory of the Abipons, that all the Spaniards and Indians in that part of Paraguay unanimouſly aſcribe a like property to the ſnake called *ampalabas*. B.

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tain circumſtances, emits from its body a ſtupefying vapour; and that it is this which produces the effect. Others are ſaid to have remarked that this terrible animal, on ſuch occaſions, makes a noiſe with the rattle in its tail, a ſingularity which excluſively belongs to it; and that there is reaſon to ſuppoſe that the whole charm is to be aſcribed to the terror produced by the ſound of that organ. Againſt theſe three ways of accounting for this phenomenon Dr. Barton makes the following objections: In regard to the firſt, the common ſymptoms after the bite of the rattle-ſnake are very different from thoſe which have been remarked in ſmall animals ſuppoſed to have been faſcinated. Beſides, this pretended faſcination can be deſtroyed, and the animal ſaved, if the ſnake be immediately killed near it; and in the laſt place, this ſo called faſcinating power is aſcribed to many ſerpents that are not poiſonous, as for example the *coluber conſtrictor*.

He endeavours to weaken the ſecond mode of explanation by remarking, that many obſervers have ſeen nothing of that mephitic evaporation of the rattle-ſnake; that ſmall birds, when encloſed in a box with the animal, have remained lively and well; and that the rattle-ſnake often lies whole days under buſhes and trees, while thruſhes and fly-catchers neſtle in them without any danger*.

As Dr. Barton's argument againſt the third manner of accounting for this property is particularly and ſolely directed againſt a paſſage in my Manual of Natural Hiſtory, I ſhall firſt quote the paſſage itſelf, before I offer any remarks on his objections.

"That ſquirrels, ſmall birds, &c. fall down ſpontaneouſly from trees into the mouth of the rattle-ſnake, lying below them, is an undiſputed fact, and is the leſs ſurpriſing, as the like phenomena have been remarked in regard to other ſnakes, and alſo toads, hawks, and cats; all of which, in

* It is a pity that Dr. Barton did not compare with his obſervations Kalm's account in the Swediſh Tranſactions for 1752, and the valuable eſſay of Michaëlis on this ſubject in the Göttingen Magazin for January 1785.

certain

certain circumſtances, as appears, have the power of drawing towards them ſmall animals, merely by fixing their eyes ſtedfaſtly on them. In regard to the rattle-ſnake, this effect is produced by the rattle in its tail, the hiſſing noiſe of which makes ſquirrels, &c. whether through curioſity, miſtake, or terror, ſeem to approach the animal as it were ſpontaneouſly. At any rate, I know from the information of intelligent eye-witneſſes, that it is a common ſtratagem of the young ſavages in America to conceal themſelves in the buſhes, where they imitate the hiſſing noiſe of the rattle-ſnake, and by theſe means attract ſquirrels, which they are then enabled to catch."

Dr. Barton's objections to this paſſage are as follows: "1. This faſcinating power is by no means peculiar to the rattle-ſnake." This is literally what I ſaid myſelf, with the addition, that the effect is produced by the rattle. This lazy animal, when lying on the ground, might certainly employ that ſingular organ for enticing animals, as well as the ceraſtes employs its horns for the ſame purpoſe, at leaſt according to common report.

2. The author ſays he has been aſſured by ſome perſons that "the rattle-ſnake, during the time of its ſuppoſed faſcination, does not employ its rattle." It is very probable that the caſe here may be altered by circumſtances. Dr. Barton, however, was told the contrary by other perſons. A Mohigan Indian informed him that the rattle-ſnake faſcinates ſquirrels and birds by means of its rattle; and by its noiſe can bring down animals from trees. An interpreter, who enabled him to carry on a converſation with a Choktaw Indian, told him the ſame thing; and the experiments of M. Voſmaer are well known.

3. In regard to the ſtratagem of the ſavages, Dr. Barton knows nothing of it; and others, of whom he made enquiry, were equally ignorant on the ſubject. He is therefore inclined to believe that I have been impoſed on, or that this ſtory perhaps has taken its riſe from the following circumſtance;

cumſtance:—The young Indians place a reed croſs-wiſe in their mouth, and by a tremulous motion of the lips imitate the cry of young birds, by which means they entice the old ones, ſo that they can then eaſily ſhoot them. In this manner the butcher bird (*lanius excubitor*), concealed in the thickeſt buſhes, imitates the cries of young birds, and thus often catches the old ones, who approach deceived by the ſimilarity of the ſound."

As far as I know, Dr. Mead, about fifty years ago, when controverting the idea that providence had furniſhed the rattle-ſnake with its rattle to give warning to travellers, was the firſt who aſſerted that it ſerves the animal to terrify ſquirrels and ſmall birds, which are then ſo ſtupefied by the ſight of an enemy ſo terrible to them, that they at length drop down and become its prey, and that this is what the Indians call faſcination. He himſelf obſerved, that when a hawk was perched on a tree in a garden, the ſmall birds in the neighbourhood were ſo ſtupefied that they fluttered about within a ſmall circle, but were not in a condition to eſcape from the claws of the ravenous animal. This accords perfectly with what Dr. Barton ſays himſelf, in general, that nature has taught different animals what kind of animals their enemies are, and that if ſmall animals are attracted by the rattle-ſnake it may be owing to fear. In regard to the pretended effect of the noiſe occaſioned by the rattle, nothing can agree better than what Dr. Barton ſays himſelf in regard to the ſtratagem of the young Indians with a reed in their mouth.

I ſhall here obſerve, that I obtained my information from Major Gardner, who, with his family, reſided many years in Eaſt Florida. He is a very intelligent naturaliſt, an accurate obſerver, and certainly would be very far from impoſing upon me.

Dr. Barton, after endeavouring by the above objections to refute the before-mentioned three methods of accounting for the faſcinating power of the rattle-ſnake, gives himſelf a

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fourth method; the ſum of which is, that thoſe birds which under certain circumſtances, as has been aſſerted, flutter into the mouth of the animal, are in general thoſe which neſtle on the ground, in buſhes, or on low trees, and which having eggs or young in their neſts, expoſe their lives through love for their brood on the approach of this dangerous enemy.

With this method of explaining the phenomenon I have been acquainted ſince 1785, from the before-mentioned Eſſay of Michaëlis, who, among many ways of ſolving the problem, gives the following:

"Others believe that it is owing merely to the care of the old ones for their young, which throw themſelves between the latter and their enemies, and by theſe means become a prey to them. One of my friends, Mr. David Colden, at Fluſhing, an amateur of natural hiſtory, and ſon of Governor Colden, whoſe ſervice to ſcience is ſo well known, aſſured me that he had ſeveral times ſeen birds faſcinated by ſnakes, but always found the neſt of the bird either with eggs or young ones in the neighbourhood, which made the ſpectators give up the idea of faſcination. But, Mr. Michaëlis adds, I know ſome inſtances where no neſt could be in the neighbourhood, and where, though the ſnake was at firſt at a great diſtance from the bird, it nevertheleſs fell towards it."

I would however add another mode of explanation from Dr. Barton's work itſelf, where he aſſures us that the reſult of his enquiries, whether the rattle-ſnake creeps up trees or not, induces him to believe the latter to be the caſe. He had an opportunity of ſeeing many of them, but he always obſerved them on the ground. They never move, like the moſt part of the other ſerpents, in a ſpiral manner, but ſtraight on; and this is the reaſon why they cannot aſcend trees. The rattle-ſnake is alſo one of the lazieſt of all the ſerpent tribe; under theſe circumſtances it ſeems very natural that ſuch a lazy animal ſhould be endowed with the

faſcinating power of bringing down from trees ſmall animals, which otherwiſe would have nothing to fear from a ſnake that cannot creep upwards.

VI. *Account of a new Method to cure and prevent the Plague, as practiſed at Smyrna. Publiſhed by Count* VON BERCHTOLD. *From the* Verkundiger, *Nov.* 8, 1798.

COUNT LEOPOLD VON BERCHTOLD, well known by his zeal for promoting every thing that can tend to public utility, when at Hamburgh, in the month of July laſt, preſented, among other works, to the ſociety for improving the arts, a ſmall treatiſe, written by himſelf, on the cure and prevention of the plague*. The ſociety, in compliance with the benevolent wiſh of the author, have publiſhed the following extract from it, which on account of its general importance to mankind, and particularly to Britain, as a commercial nation, we hope our readers will thank us for laying before them.

In the preface, Count von Berchtold pays a handſome compliment to the humanity of Mr. Baldwin, the Engliſh conſul, who during a long reſidence in the Turkiſh empire made very accurate obſervations on the nature of the plague, and endeavoured to diſcover ſome efficacious means of guarding againſt that dreadful and deſtructive ſcourge. After long labour he at length found that this effect may be produced by olive oil. Perſons infected by the plague, and given over by the phyſicians, were perfectly cured by

* The title is as follows: Deſcription of a new Method to cure and prevent the Plague, which was employed with the beſt Succeſs in the Hoſpital of St. Anthony at Smyrna. Collected in that City, and publiſhed by Leopold von Berchtold, Knight of the Military Order of St. Stephen in Tuſcany. To be diſtributed Gratis, for the Benefit of thoſe Nations which trade in the Levant. Vienna, 1797. 28 pages octavo.

rubbing their bodies over with warm oil; and others not yet infected were preſerved from it by the ſame means. Experiments were made, by Mr. Baldwin's deſire, in the hoſpital of St. Anthony at Smyrna; and P. Luigi di Pavia*, chaplain to the hoſpital, who for twenty-ſeven years has been of ſo much ſervice during periods when this diſeaſe prevailed, aſſerts that of all the remedies he ever employed, olive oil has been the moſt effectual. The following information, in regard to the manner of employing this remedy and preventive, was communicated to Count von Berchtold by the above P. Luigi.

This excellent cure for the plague conſiſts in rubbing olive oil, with the ſtrongeſt friction, into the whole body of the infected perſon. When the body is thus rubbed, the pores being opened imbibe the oil, and a profuſe perſpiration takes place, by which the poiſonous infection is again thrown out. This operation muſt be performed the firſt day of the infection; and if only a weak perſpiration enſues, it muſt be repeated till it is obſerved that every particle of infection is removed, and that the whole body of the patient is covered with a profuſe ſweat. Neither the patient's ſhirt nor bed-clothes muſt be changed till the perſpiration has

* Mr. Howard, in his work on Lazarettos, page 32, ſays: "Fra. Luigi de Pavia, prior of the hoſpital of San Antonio of Smyrna, is a native of Padua, of the order of Recolets. He built and eſtabliſhed an hoſpital about twenty-ſeven years ſince, and has applied the whole of the penſion he receives from his family to its benevolent purpoſes. Patients of all ranks are admitted without a fee; and what is contributed by thoſe of the better ſort is added to the common ſtock. *He does not pretend to any ſkill in medicine, but tries every plauſible experiment with unremitted attention*, and frequently performs the moſt menial offices himſelf. Having been infected himſelf with the plague, he made a vow to attend one perſon at leaſt, if he recovered. His ſucceſs has induced him to dedicate the remainder of his life to that ſervice. He has lately adopted the *oiled ſhirt with friction, and found it a remedy of more frequent avail than many others*. He computes, with the ſtricteſt veracity, that of his patients nearly two thirds have eſcaped death." EDIT.

entirely ceaſed. The operation muſt be performed in a very cloſe apartment; and at every ſeaſon of the year there muſt be kept in it a fire-pan, over which ſugar and juniper muſt be thrown, from time to time, that the vapour which thence ariſes may promote the perſpiration. The whole body of the patient, the eyes alone excepted, muſt in this manner be anointed, or rather rubbed over with the greateſt care.

In the ſpace of a year I remarked that this operation, if not employed too late, was attended with the moſt wonderful effect. If it be delayed till the nervous ſyſtem of the patient is attacked by the diſeaſe and the blood corrupted, after which a decompoſition of it follows, the remedy is of no avail, and the patient muſt fall a victim. The operation, therefore, muſt be performed as ſoon as the ſmalleſt ſymptom of infection is remarked. In caſes where perſpiration was ſlow, I have endeavoured to promote it by making the patient drink tea of elder flowers without ſugar, from which I have ſeen the happieſt effects.

To the above rules Count von Berchtold adds the following remarks by way of illuſtration: 1. The operation of rubbing in the oil muſt be performed by means of a ſponge, and ſo ſpeedily as not to laſt more than about three minutes. 2. The interval between the firſt and the ſecond rubbing, if a ſecond be neceſſary, muſt be determined by circumſtances, as the ſecond muſt not be performed till the firſt perſpiration is over, and this will depend on the conſtitution of the patient. If any ſweat remains upon the ſkin, it muſt be wiped off with a warm cloth before the ſecond rubbing takes place. This ſtrong friction with oil may be continued, for ſeveral days ſucceſſively, until a favourable change is remarked in the diſeaſe; after which the rubbing may be performed in a more gentle manner. The quantity of oil requiſite each time cannot be determined with accuracy; but, in general, a pound may be ſufficient. The pureſt and freſheſt oil is the beſt for this operation: it muſt not be hot, but only luke-

warm,

warm. The breaſt and privities muſt be rubbed ſoftly. In a cold climate ſuch as ours, thoſe parts only into which the oil is rubbed muſt be expoſed naked. The other parts muſt be covered with warm clothing. In this manner each part of the body muſt be rubbed with oil in ſucceſſion, as quickly as poſſible, and be then inſtantly covered. If the patient has boils or buboes, they muſt be rubbed over gently with the oil till they can be brought to ſuppurate by means of emollient plaſters. The perſons who attend the patients to rub in the oil muſt take the precaution to rub themſelves over in the like manner, before they engage in the operation. They muſt, if poſſible, avoid the breath of the patient, and not be under any apprehenſions of catching the infection.

P. Luigi then ſays: In order to prevent the patients from loſing their ſtrength, I preſcribed for them, during four or five days, ſoup made of vermicelli boiled in vinegar without ſalt. I gave them ſix or ſeven times a day a ſmall ſpoonful of preſerved ſour cherries; preſerved not with honey, but with ſugar, as the former might have occaſioned a diarrhœa. When convinced that the patients were getting better, I uſually gave them the fifth morning a cup of good Mocha coffee, with a piece of toaſted biſcuit (*biſcotto*) prepared with ſugar, and I doubled the latter according to the ſtrength and improvement of my patients.

At dinner and ſupper I allowed them for 15 or 20 days nothing but rice or vermicelli boiled in water. After that period, I increaſed the quantity of bread in proportion to their appetite. In ſummer they had ſoup made of ſmall pompions, and of herbs in winter, without any other addition than a little oil of almonds. I gave them alſo, according to their condition, either ſome oranges or a very ripe pear, boiled if in winter, and as much toaſted bread as might enable them to digeſt their food eaſily and leave them ſome appetite remaining. After the thirtieth or thirty-fifth day I allowed them morning and evening ſoup made from a fowl or neck of lamb; but I did not allow them fleſh till after the fortieth

day, leſt it might occaſion indigeſtion, which is very dangerous and often mortal in this diſorder, and give riſe to a new breaking-out of peſtilential boils. After the fortieth day I allowed them to eat boiled as well as roaſted veal, and to drink moderately of wine; but I recommended to them to abſtain from fiſh, ſalted proviſions, pork, four fruits, and, in general, every thing that either by quantity or quality might occaſion indigeſtion.

This regimen has been the reſult of obſervations made in the courſe of twenty-ſeven years, during which I have attended patients attacked by the plague. It has been approved by the ableſt phyſicians; and long experience has ſhewn that it is the beſt calculated to guard againſt the dangerous conſequences of this horrid diſeaſe.

According to the accounts of Mr. Baldwin, among the thouſands who died of the plague in Upper and Lower Egypt, there was not one perſon who had been employed in carrying oil; from which I concluded that oil muſt alſo be a preſervative againſt it. I therefore put in practice this idea in regard to all thoſe perſons who were infected without knowing it, and I always found it infallible in its effects, as will appear from the following ſtriking proofs:—In the year 1793, twenty-one Venetian ſailors reſided, day and night, for five days, with three perſons ill of the plague, who afterwards died, and preſerved themſelves from infection by often rubbing with oil in the manner above directed. The ſame year three Armenian families, one conſiſting of thirteen, the ſecond of eleven, and the third of nine perſons, remained with ſome of their relations ill of the plague; and though they lay in the ſame apartment with them, according to their cuſtom, and even in the ſame bed, and held the ſick in their arms, none of them were attacked by the diſeaſe, becauſe, in conſequence of my advice, they daily rubbed their bodies over with oil. In the year 1794, a healthy poor woman attended thirteen perſons ill of the plague; remained with them day and night, and yet was preſerved from this

horrid

horrid diſeaſe by anointing herſelf with oil. The ſame year two perſons attacked by the plague remained ſeveral days in the ſame houſe with the numerous family of M. Natale Pezer, a Raguſan; but the family employed oil, as above mentioned, and were perfectly free from infection.

In a word, this wonderful preſervative has ſo much reputation at Smyrna, that almoſt all the inhabitants, though perfectly ſound, when the unfortunate infection makes its appearance, immediately ſecure themſelves againſt it by repeated rubbing with oil.

When this operation is uſed as a preventive, it is not neceſſary that it ſhould be performed in the ſame manner as on thoſe infected. As it is not done for the purpoſe of promoting perſpiration, but merely to prevent the infection from penetrating through the pores, it is not requiſite that it ſhould be performed with the ſame ſpeed as when for curing the diſorder; nor is it neceſſary to abſtain from fleſh and to uſe ſoups; but it will be proper to uſe only fowls or veal for ten or twelve days boiled or roaſted, without any addition or ſeaſoning *(condimento)*. In the laſt place, it will be neceſſary to guard againſt fat and indigeſtible food, and ſuch liquors as might put in motion or inflame the maſs of the blood.

Careful and repeated rubbing of the whole body with oil prevents the breaking out of new boils, which often take place after the oil has been firſt uſed. The oil muſt be applied as ſoon as poſſible after the infection has been communicated: if omitted for four or five days, it will be of no effect. Diarrhœa in this diſeaſe is a ſymptom of death, and difficult to be cured; but the editor adds, in a note, that Father Luigi, by the uſe of oil, was enabled to recover four perſons who had been already attacked by it. The abundant perſpiration produced by the oil not only prevents new buboes from breaking out, but in a few days brings thoſe which have appeared to ſuppuration, or makes them vaniſh entirely.

In the courſe of five years, during which friction with oil was employed in the hoſpital at Smyrna, of 250 perſons attacked by the plague the greater part were cured; and this would have been the caſe with the reſt had they not neglected the operation, or had it not been employed too late after their nervous ſyſtem had been weakened by the diſeaſe ſo as to render them incurable. Immenſe numbers of people have been preſerved from the effects of this malady by the above means; and of all thoſe who have anointed themſelves with oil and rubbed it well into their bodies, not one has been attacked by the plague, even though they approached perſons already infected, provided they abſtained from heavy and indigeſtible food.

The above account of P. Luigi is accompanied with ſeveral atteſtations of the efficacy of oil, both as a cure and a preventive, ſigned by B. Giraud the Imperial conſul, and F. Merry the Engliſh conſul, at Smyrna.

We are informed by Count von Berchtold, that this method of cure is at preſent employed with the beſt effects in Egypt, Conſtantinople, and Wallachia; and that great care has been taken to make it generally known both in the Eaſt and in thoſe countries that carry on trade in the Levant.

This important diſcovery, which does ſo much credit to our countryman Mr. Baldwin, deſerves the ſerious conſideration of all medical men; for, if olive oil has been found efficacious in curing or preſerving againſt one ſpecies of infection, it is not abſurd to ſuppoſe that the ſame, or other kinds of oil, might be productive of much benefit in other malignant infectious diſeaſes. We hope ſoon to hear of ſome trial being made with it in this country. Would it be of any ſervice in the yellow fever, ſo prevalent in the weſtern world?

VII. *Examination of the Experiments and new Obſervations of* G. PEARSON, *M.D. on Human Urinary Concretions*; and a Compariſon of the Reſults obtained by that Chemiſt with thoſe of* SCHEELE, BERGMAN, *and ſome of the French Chemiſts. By C.* FOURCROY. *From* Annales de Chimie, *Vol. XXVII.*

DR. George Pearſon, an Engliſh chemiſt, well known by his very uſeful labours, and particularly his ingenious experiments on the decompoſition of the carbonic acid, in his *Experiments and Obſervations tending to ſhew the Compoſition and Properties of Urinary Concretions*, inſerted in the Philoſophical Tranſactions for 1798, endeavours to prove that the calculi of the human bladder are compoſed of a particular matter hitherto not well known to chemiſts; which Scheele, according to his opinion, neglected to ſtudy, and which he calls the *uric* oxyd. As this concluſion tends to overturn completely the diſcovery of the illuſtrious Swede, confirmed by Bergman, reſpecting the acid of the human urinary calculus, and the reſult of very numerous experiments which I ſince made on that animal concretion, (a reſult which ſerved to confirm in many points the firſt data of Scheele, according to which the expreſſion of lithic acid had been adopted in the new nomenclature,) I thought it my duty to examine with particular care the long diſſertation of Dr. Pearſon. I was beſides induced to do ſo by the lively intereſt which it is pretty generally known I take in every thing that regards the analyſis of animal ſubſtances; and the more ſo at preſent, on account of the reſearches reſpecting urine on which I and Cit. Vauquelin have been for ſeveral years employed. Inſtead of finding ourſelves anticipated on this ſubject by this able Engliſh chemiſt, I was not a little aſtoniſhed to diſcover in his diſſertation a great number of aſſer-

* For Dr. Pearſon's paper on this ſubject, ſee Philoſoph. Mag. vol. ii. p. 38 and 130.

tions and facts which ſhew, that the author ſeems not to have accurately comprehended the labours of Scheele, and that he ſeems to have overlooked the reſearches which I publiſhed on the ſame ſubject, though he quotes me in regard to ſome facts, but only as one of thoſe who may be conſidered as almoſt a ſtranger to the object which he had in view. It alſo appears to me, that the experiments he made, inſtead of contradicting the facts obſerved by Scheele and myſelf, and the conſequences I have thence deduced in regard to the nature of urinary concretions, are rather calculated to confirm them.

What I mean to eſtabliſh in this memoir requires, 1ſt, That I ſhould give an exact account of Dr. Pearſon's experiments: 2d, That I compare with his reſults the facts obſerved by Scheele and Bergman; and that I add to them thoſe which I obſerved myſelf: and, 3d, That I draw from this compariſon thoſe inductions which form the principal part of this memoir.

The firſt pages of this diſſertation contain a very conciſe account of the labours of Scheele, Bergman, Fourcroy, Link, and Black. But I muſt here obſerve, that this ſhort notice is far from being ſufficient to give an accurate idea of theſe labours; that it is very ſingular to hear Dr. Pearſon ſay, that the particular matter of which he intends to ſpeak, and which he ſays he obtained by diſſolving concretions in ley of fixed cauſtic alkali and precipitating it by means of acids, was conſidered as of little importance by Scheele, who, he ſays, of courſe never made any reſearches in regard to its properties; whereas, if we read with attention the maſterly diſſertation of the Swediſh chemiſt, we ſhall find that it was this matter which he conſidered as his real acid of calculus, and which we have ſince called the *lithic acid*. I myſelf have ſeveral times announced in my works, particularly under the article of calculus in the *Dictionnaire de Chimie Encyclopedique*, inſerted in the *Annales de Chimie*, 1793, tom. xvi. that it was thus that I prepared the lithic

acid

acid for my lectures. Nor is it accurate to ſay, that the acid ſublimate obtained by Scheele by treating the urinary calculi by fire, was taken for the ſame as the precipitate of their ſolution in fixed alkali by an acid, ſince the French chemiſts, in announcing that a portion of the lithic acid was ſublimated without decompoſition, propoſed the proceſs of ſublimation to obtain the lithic acid; and ſince I have particularly deſcribed the decompoſition of the greater part of this acid by fire, when I announced its property of being convertible into the Pruſſic acid. But I ſhall now proceed to detail the experiments, as I ſhall have occaſion ſeveral times to ſhew that Dr. Pearſon has been ſingularly miſled in regard to our lithic acid.

[Cit. Fourcroy then gives a pretty full account of Dr. Pearſon's experiments, which it would be uſeleſs to repeat here, as we have already in the laſt two numbers of the preſent work laid the paper at full length before our readers. On Dr. Pearſon's expreſſion (Phil. Mag. vol. ii. p. 130), "I "ſhall next relate ſome experiments made in order to ob- "tain the acid ſublimate of Scheele, or lithic acid of the "new ſyſtem of chemiſtry," the French chemiſt makes the following remark: "I muſt obſerve that theſe words prove that Dr. Pearſon did not underſtand very clearly the reſults of Scheele and of the French chemiſts; for neither the one nor the other have ever conſidered as real lithic acid the product ſublimated by fire from the urinary calculus, but only the matter of the whole calculus precipitated in cryſtals from the boiling water with which it has been treated, or from its ſolution in fixed cauſtic alkali by an acid. I muſt remark here alſo, that Scheele in his diſſertation has accurately diſtinguiſhed this ſublimated portion from the real acid of the urinary calculus not treated by fire; that he has compared it to the volatile acid ſalt of amber; and that Cit. Vauquelin, in an article which he furniſhed for Deſchamp's *Traité de la Lithotomie*, vol. i. p. 132, &c. took great care to point out that the ſublimated produce of the calculus of the blad-

der was nothing but the matter of the calculus altered and changed in its nature."—Citizen Fourcroy concludes his abridgement of Dr. Pearſon's paper as follows: "I thought it my duty to give an account of all his [Dr. Pearſon's] experiments and reſults, in order that it might be more eaſy to compare what he has done with the reſearches of Scheele and thoſe made by myſelf. By giving a ſimilar abridgement of the latter alſo, it may be then ſeen whether Dr. Pearſon has made uſeful additions to the diſcoveries which preceded his labour, and whether the concluſions which he thinks himſelf authoriſed to deduce from his experiments can be found in them." Cit. Fourcroy then proceeds as follows:]

A ſhort Account of the Reſearches of Scheele and Bergman, and of thoſe made by myſelf.

When the illuſtrious Scheele read in 1776, to the Academy of Stockholm, his examination of the bezoar, or ſtone of the human bladder, no one then had an accurate idea of the nature of this concretion, though Margraf had already obſerved, that it was not formed of an abſorbing earth, as was pretty generally believed before him, and as has been repeated ſo often ſince in many works on medicine. Scheele obſerves in the beginning of his diſſertation, that he examined ſeveral calculi, ſmooth, rough, or angular; that he found them poſſeſs the ſame nature and properties: it is therefore the hiſtory of the genus that he meant to give. His memoir is divided into ten ſections. To aſcertain properly the value of his labour, I muſt here make the reader follow him through theſe ſections.

I. The ſulphuric acid diluted produces no effect on the calculus; but concentrated by diſtillation it diſſolves it: the ſolution diſtilled to dryneſs leaves a black coal, giving off ſulphureous acid fumes.

II. The muriatic acid, diluted or concentrated, has no effect ever by ebullition on the urinary calculus.

III. Weak

III. Weak nitric acid attacks it cold: with the aſſiſtance of heat there is produced an efferveſcence and red vapour; the calculus is entirely diſſolved, and carbonic acid is diſengaged. This ſolution is acid, even when ſaturated with the calculus: it gives a beautiful red colour to the ſkin in half an hour; when evaporated it becomes of a blood red; a drop of acid then added deſtroys the colour; it does not precipitate the muriat of barytes nor metallic ſolutions, even with the addition of an alkali; the latter makes it turn yellow; a ſuperabundance of alkali gives it, by digeſtion, a roſe colour, as well as the property of precipitating ſulphat of iron black, ſulphat of copper green, nitrat of ſilver grey, the ſuperoxygenated nitrat of mercury and ſolutions of zinc and lead white. The ſame nitric ſolution of the calculus of the bladder gives, with lime water, a white precipitate, which diſſolves without efferveſcence in the nitric and muriatic acids without deſtroying their acidity. The oxalic acid added to the nitric ſolution gives no precipitation.

IV. Potaſh united to the carbonic acid does not diſſolve the urinary calculus, either hot or cold; but a perfectly cauſtic ley of the ſame alkali, without any trace of the carbonic acid, diſſolves it even cold. This ſolution is yellow, of a ſweetiſh taſte; it is precipitated by all the acids, even the carbonic; it does not render lime water turbid; it decompoſes and precipitates metallic ſolutions, thoſe of iron brown, of copper grey, of ſilver black, of zinc, mercury and lead white: it exhales an odour of ammonia.

V. Lime water diſſolves the calculus by digeſtion; 200 parts almoſt are neceſſary to take up one; it then loſes its ſharp taſte; this ſolution is precipitated in part by acids.

VI. Pure water diſſolves entirely the ſtone of the human bladder; but it is neceſſary to boil, for ſome time, 360 parts with one part of the calculus reduced to powder to effect a ſolution. This ſolution, being then complete, reddens a tincture of turnſole; does not render lime water turbid;

and

and on cooling depoſits almoſt every thing it contains in ſmall cryſtals.

VII. Seventy-two parts (grains) of this calculus, diſtilled in a ſmall glaſs retort over an open fire urged ſo as to bring the veſſel to a red heat, produced water of ammonia like hartſhorn, and a brown ſublimate without oil: there remained twelve parts or $\frac{1}{6}$ of a black charry ſubſtance, which preſerved its colour on red hot iron in the open air. The brown ſublimate, weighing 28 parts, grew white by a ſecond ſublimation; it was deſtitute of ſmell, even when moiſtened by an alkali; its taſte was acid; it diſſolved in water by ebullition; alcohol diſſolved it alſo, but in leſs quantity than water: it did not precipitate lime water, and reſembled the ſuccinic acid.

From the facts in theſe ſeven ſections, Scheele concludes that the urinary calculus does not contain either ſulphuric acid or lime; but that it is compoſed of a concrete acid, oily, volatile, mixed with a little gelatinous matter, which acquires new properties and changes its nature by the nitric acid.

VIII. He ſays he found a little of this acid in all urine, even in that of children. This liquor evaporated to $\frac{1}{112}$ of its weight (14 pounds reduced to 2 ounces) depoſits a ſubtle powder ſimilar to the calculus, which adheres to the veſſel, and which the cauſtic alkali diſſolves very eaſily. The depoſit from the urine of patients labouring under fevers exhibited the ſame nature; it is formed in cloſe veſſels as well as in thoſe that are open; it is re-diſſolved by means of heat, and its precipitation is owing only to the cooling of the urine.

IX. All urines thus contain phoſphat of lime, kept in ſolution by an exceſs of the phoſphoric acid; which is the reaſon that it reddens blue paper, and depoſits a white powder by means of ammonia. Urine gives of it $\frac{1}{576}$ of its weight. This precipitate diſſolved in the nitric acid is rendered

rendered turbid by the addition of the ſulphuric acid, which forms with it a ſulphat of lime; the ſupernatant liquor, when evaporated, leaves the phoſphoric acid after ſeparating the nitric acid by evaporation. The urine of diſeaſed perſons is more acid, and contains more phoſphat of lime, than that of healthy perſons.

X. It reſults from all theſe facts, ſays Scheele in concluding his diſſertation, that urine, independently of the ſubſtances already known, viz. the muriats of potaſh, ſoda, and ammonia, the phoſphat of ſoda and ammonia, and an oily extractive matter, contains a concrete acid, hitherto unknown, (which forms the urinary calculus,) and phoſphat of lime.

I have mentioned with accuracy, and without omitting a ſingle experiment, the labour of Scheele, becauſe it appeared to me neceſſary to do ſo, in order to eſtabliſh an eaſy and preciſe compariſon between his experiments and thoſe of Dr. Pearſon.

The diſcovery of Scheele, communicated to the Academy of Stockholm, was confirmed by Bergman, who gave, under the title of an addition, a memoir containing his own experiments on the ſtone of the human bladder, with which he was occupied at the ſame time as his pupil and friend; and it may be readily perceived what muſt be the weight of the aſſent of that celebrated man, who firſt introduced into the deſcription of chemical phenomena, and the reaſoning on them, the purity, preciſion, and method of the geometricians. In announcing that his experiments conducted him to the ſame concluſion, viz. that the urinary concretion was compoſed chiefly of a particular acid; he ſays he found ſome differences, which, though he aſcribes them to thoſe of ſubſtances which they had both examined, were, however, found in all thoſe which he treated. It is of importance that I ſhould relate here the differences obſerved by Bergman, becauſe they may ſerve to explain thoſe which were obſerved by Dr. Pearſon himſelf, and which led him to

draw

draw a concluſion contrary to that of the illuſtrious Swediſh chemiſts whom he combats in his memoir.

1. Bergman never ſucceeded ſo far as to diſſolve completely urinary concretions in diſtilled water, or in the nitric acid; but at the ſame time the greater part was diſſolved in the latter, eſpecially when precautions were taken to employ an exceſs of the ſolvent, fragments of calculus inſtead of powder, a veſſel ſufficiently ſmall to permit the inſoluble matter to be collected; and by keeping the matters at a heat near that of boiling water, there was ſeparated a white ſpongy matter, on which water, alcohol, acids, and ammonia had no action; dividing itſelf into very minute particles by ebullition, though ſtill inſoluble; giving by fire a coal difficult to be incinerated, and of which the aſhes were inſoluble in the nitric acid. He never had enough of this ſubſtance to enable him to become better acquainted with its nature.

2. Though the nitric ſolution of the calculus is not precipitated by the oxalic acid, Bergman thinks that lime may be found in it in union with ſome matter that prevents it from being precipitated; and he finds a proof of this in the aſhes of burnt urinary calculus, which exhibited the ſame phenomena as lime in his experiments. The nitric ſolution of calculus evaporated to dryneſs, and burnt or calcined to whiteneſs, gave him alſo traces of lime. The concentrated ſulphuric acid thrown into this ſolution when evaporated, exhibited likewiſe ſmall cryſtals of the ſulphat of lime. This earth rarely exceeds $\frac{1}{200}$ in the urinary calculus.

3. The concentrated ſulphuric acid diſſolves the calculus with effervеſcence by the means of heat; it becomes of a blackiſh brown colour: a little water added to this ſolution makes it immediately coagulate; a great deal of this liquid re-diſſolves the whole coagulum, and forms a ſolution of a yellowiſh brown colour. The muriatic acid has no action on the urinary calculus, and deprives it of no part of its lime.

4. Berg-

4. Bergman frequently mentions the red colour aſſumed by the nitric ſolution of the urinary calculus when ſpontaneouſly evaporated in the air, or by the action of fire; even when it does not contain an exceſs of free nitric acid: he obſerves that this colour diſappears by the addition of acids and alkalies; that it becomes black by the action of a ſtrong fire; that the calculus thus diſſolved and evaporated is afterwards ſoluble in all acids; that by employing the nitric acid too much concentrated, the calculus is wholly reduced into froth; that the ſudden ſwelling-up of the ſolution when rapidly heated is conſiderable, and the foam of a bright red; that alkalies do not ſeparate the calculus from the nitric acid, but unite with it as they do when two acids are preſented to them; that the red maſs obtained by evaporation of the nitric ſolution is very different from the concrete acid of the calculus in its colour, its deliqueſcence, the roſe-coloured tint which the ſmalleſt quantity of it communicates to water, its ſolubility, and its loſs of colour by the muriatic acid, the ſpots which it leaves on the ſkin, on bones, glaſs, paper, eſpecially by the aid of time and heat. He aſcribes theſe remarkable effects to the alteration produced in the native acid of the urinary calculus by the nitric acid, rather than to a precipitation with the latter.

5. Bergman, in mentioning that he made many other experiments on the ſtone of the bladder, takes care to point out that they prove nothing more or any way different from what Scheele has ſaid in his excellent memoir. All reſearches, ſays he at the end of his addition, for the purpoſe of diſcovering a remedy for this diſeaſe, ought to be founded on a perfect knowledge of the properties of the calculus. He obſerves that alkalies are the only truly active remedies, the efficacy of which has been acknowledged by medical experience, in concert with chemical reſearches. He concludes his note by announcing that he hoped to be able to determine more accurately whether all calculi of the bladder were really of the ſame nature. But during the eight years which

which were added to his life after this epoch 1776 (he died in 1784) his occupations and deranged ſtate of health prevented him from completing this labour, as he wrote nothing more than what I have here made known.

[To be concluded in the next Number.]

VIII. *Obſervations on the Garden Spider, and the Method it employs to conſtruct its Web. By an anonymous Author. From the* Journal de Phyſique, *Vendemiaire, An. VII.*

THOUGH the natural hiſtory of inſects be not the particular object of my ſtudies, I never neglect any facts, relating to that ſcience, with which accident may make me acquainted. In the month of Germinal laſt I had before my windows ſome ſhrubs in pots; and one day, when it was very warm, I removed them from the ſun, and placed them on the floor of my chamber. Caſting my eyes on them about an hour after, I was not a little ſurpriſed to ſee a pretty, ſmall, vertical ſpider's web, which extended from one ſhrub to another. It was not above two inches in diameter; but the concentric circles and radii were exceedingly numerous. Nothing could be more elegant; and the centre of it was quietly occupied by a ſmall ſpider, not larger than the head of a pin. I was certain that this web had not exiſted an hour before, as I had removed my pots one after the other; and I could not conceive how ſo much work, that muſt have required ſo many journeys, could have been completed ſo ſoon; but what increaſed my aſtoniſhment not a little was, that each pot was placed in a ſmall veſſel filled with water. Suppoſing then that the ſpider had gone from the one ſhrub to the other to fix its threads, it muſt have been obliged to croſs the water, which appeared to me improbable, as the animal was not of the aquatic kind.

I then recollected a ſimilar fact, which I had obſerved the

year

year before in company with some of my friends, and which had appeared to us equally wonderful. We were walking in the gardens of a country-seat, much neglected for several years, where we saw an alley of horse-beeches, the branches of which left a passage of scarcely five or six feet, barred up, as it were, by a multitude of spiders' webs in a vertical position. As we began to enquire how the spiders had been able to convey their threads from one side of the alley to the other through so many leaves and branches, one of the company said that the threads, in all probability, had been floated to the other side by the wind, and that they had been caught and retained by the opposite branches. The problem seemed thus resolved, and the conversation turned upon some other subject. The fact, however, which had taken place in my chamber, where the air was in a state of perfect tranquillity, clearly convinced me that the threads of these insects had not been transported by the wind; and that they must have some direct method of conveying them from one place to another.

I was then induced to suppose that these spiders were themselves acquainted with some method of projecting their threads, from the point where they are stationed, to another at which they wish to arrive. In consequence of this idea, I resolved to make some experiments. I removed, by means of a feather, the small spider from the middle of its web; and to be sure that no floating thread adhered to it, I moved another feather several times around that on which the spider was placed. I then gave it a gentle shake, which made the insect descend seven or eight inches, extending its legs and spinning. It then remained stationary in a horizontal situation, having all its legs folded up on its belly, where it had applied its thread; so that it seemed to be suspended by the middle of its body. I saw it from time to time make half a turn, very speedily, sometimes to the right and sometimes to the left, and this movement was perfectly spontaneous; for there was no agitation in the air or the feather,

which I had fixed to the back of a chair. After being ſuſpended in this manner half an hour, the ſmall ſpider made a ſudden ſpring towards my breaſt, raiſing itſelf rapidly by an oblique line, which made an angle of 40 or 50° with the perpendicular. I repeated this experiment ſeveral times; and I always obſerved that the ſpider, after having remained a few moments ſuſpended, conſtantly roſe in an oblique direction, in order to reach ſome neighbouring object.

My attention being diverted by ſomething elſe, I did not then carry my obſervations any farther; but in the month of Thermidor laſt, having found under my ſhrubs one of theſe ſpiders, about the ſize of a grain of hemp-ſeed, I reſumed my former experiments; and, being furniſhed with a magnifying glaſs, I attentively examined every thing that paſſed at the moment when the ſpider was in ſuſpenſion. It was not long before I ſaw, very diſtinctly, a pretty large thread iſſue in a jet from its anus, and riſe diagonally, making with the thread of ſuſpenſion an angle of about 45°. This thread was lengthened about ſeven or eight inches, at leaſt, per ſecond. When the thread reached a neighbouring body it remained there attached; and the inſect then making half a turn, darted out another on the oppoſite ſide, and proceeded thus alternately five or ſix times. The ſpider then mounted with rapidity, and traverſed theſe different threads, which became ſtretched horizontally, by I know not what operation, though at firſt they would have formed an angle of 90°, the ſummit of which was occupied by the ſuſpended animal. Soon after I ſaw a multitude of other threads eſtabliſhed between theſe principal ones; and the work was carried on with ſo much rapidity, that it was impoſſible for me to follow it minutely. The net work ſeemed as if formed by magic; but no doubt remained to me reſpecting the principal fact, which is the emiſſion of the large tranſverſal threads, an operation not performed by chance, but deſign, and which might be compared perhaps to the extenſion of the long tentacula of certain marine animals.

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As the threads, when they iſſue from the body of the animal, aſcend always obliquely, it appears to me that this is produced by the effect of a double impulſe. On the one part the inſect forces them out horizontally, and on the other their own ſpecific lightneſs makes them mount vertically, from which there reſults an oblique direction. This ſpecific lightneſs is owing, in my opinion, to a kind of veſicle, ſhaped like a very elongated Prince Rupert's drop, which is at the end of the thread at the moment of its emiſſion. This veſicle, the ſubſtance of which ſeems to be exceedingly thin, is perhaps filled with ſome fluid, light, and at the ſame time viſcous, which poſſeſſes the double property of making the thread aſcend, and of cementing it to the bodies which it touches.

In a word, I have no doubt that the garden ſpider directs at pleaſure, and towards a determined point, thoſe threads which it throws out; for I remarked, in the alley abovementioned, that all the webs were vertical and at right angles to it, which could not be conſidered as the effect of chance. I hope, therefore, that entomologiſts will purſue theſe obſervations, in order to diſcover, if poſſible, by what ſenſe the ſpider judges either of the diſtance or the poſition of the bodies to which it directs its threads. I obſerved that the ſituation in which it was when it emitted them did not allow it to ſee the place to which they were directed. Beſides, I have ſome reaſon to doubt that the eyes of ſpiders are actually the organs of viſion. I have often preſented to theſe inſects different bodies, by which they were in no manner frightened, though exceedingly near to them, and they muſt certainly have ſeen them, had they been endowed with the faculty of ſight. I have even cut off ſome of their claws without their perceiving the ſciſſars; and yet it is well known that the miſtruſt of ſpiders is ſo great, that, as ſoon as they have the leaſt perception of a ſtrange object, they betake themſelves to flight. On the other hand, if their web be agitated in the ſlighteſt manner, as it would be by a

fly, they are ſeen to run out that moment, and never retire back until they have aſſured themſelves, by feeling, that they have been deceived. An animal endowed with ſight would not commit ſuch errors. Beſides, the curious hiſtory of their amours; the different manœuvres they practiſe before they touch each other; their fear and miſtruſt when they firſt approach, and their precipitate flight, all announce that they are very uncertain in regard to the object preſented to them: it is only by means of feeling that they are able to diſcover that they have not to do with an enemy.

My doubt on this head is not confined merely to ſpiders, but extends to all white-blooded animals, the cruſtaceous excepted. It is probable that the antennæ and other tentacula perform, in inſects and worms, the office of the organs of ſight. That multitude of eyes aſſigned to flies, butterflies, &c. in my opinion, is by no means agreeable to the progreſs of nature, which never forms any thing ſuperfluous. But what could be more uſeleſs than that profuſion of organs of the ſame ſenſe, eſpecially in beings which, by the ſhortneſs of their life, are the leaſt expoſed to the loſs of theſe organs; while the largeſt animals, deſtined to exiſt for a century, have received only a double organ of ſight, which is ſo neceſſary to them? This would be an abſurdity which we cannot ſuppoſe to exiſt.

I ſhall here add, in regard to the garden ſpider, that as Nature is infinitely wiſe, ſhe could not intruſt to chance their moſt eſſential means of exiſtence. Had ſhe made the fabrication of their web to depend on a breath of wind, that may not take place, ſhe would have expoſed them to the danger of periſhing. It was neceſſary, therefore, that ſhe ſhould put under their diſpoſition direct means; and this ſeems to be actually the caſe. But we muſt generaliſe this conſequence, and conclude, that when any mean is indiſpenſibly neceſſary for the preſervation of a ſpecies of living beings, Nature always places this mean in their own power. She has rendered it precarious at no time; and it never depends upon

upon any fortuitous circumſtances. As I have ſince learned from two well-informed naturaliſts, that it was commonly believed that the garden ſpider merely ſuffered its threads to float in the wind by chance, I thought it might be of ſome utility to publiſh the reſult of my obſervations.

IX. *On the Syſtem of Forces.* *By* J. C. Delametherie. *From* Journal de Phyſique, *Brumaire, An. VII.*

GEOMETRICIANS, accuſtomed to preciſe mathematical demonſtration, cannot reconcile themſelves to the difficulties which occur in the explanations given by phyſics to the different phenomena of nature. They prefer the method of the great Newton, which is, to calculate the effects whatever may be the cauſes; and hence has been introduced the ſyſtem of forces, which ſeems to gain ground in a ſingular manner. Kant is one of the firſt who announced it in Germany. He diſtinguiſhes two kinds of phyſics, the *dynamic* and *atomiſt*. He calls atomiſt phyſics that which teaches that the primitive parts of matter are compoſed of atoms or indiviſible parts, and which endeavours to explain all the phenomena of nature by the action or impulſe of different fluids. The ſecond, which he calls dynamic phyſics, appears to him preferable. It ſuppoſes matter to be compoſed of moleculæ always diviſible. This matter is actuated by two forces: 1ſt. the attractive; and, 2d. the repulſive. The atomiſts conſider fluids as compoſed of ſmall ſolid moleculæ. Kant does not admit theſe ſolid moleculæ. Fluids, according to him, are ſuch originally, ſo that we cannot conceive a part of a fluid which is not fluid. Theſe two forces, according to Kant, produce ſeveral other forces, viz. 3d. the caloric force, which is the principle of heat and of all the phenomena aſcribed to fire; 4th, the luminous force, which is the principle of light and all the phenomena depending on it; 5th, the electric force, which is the cauſe of all the phe-

nomena aſcribed to electricity; 6th, the magnetic force, which produces all the phenomena aſcribed to magnetiſm. Conſequently all the phenomena of heat, light, electricity, magnetiſm, according to him, are modifications of the attractive and repulſive force of the firſt parts of matter, and there exiſts neither caloric fluid, luminous fluid, electric fluid, nor magnetic fluid. This ſyſtem has numerous partiſans in Germany, and it has even been extended ſtill farther. 7th. The galvanic force. Humboldt ſuppoſes that the phenomena of galvaniſm may be explained by a particular force, which he calls the galvanic. Humboldt however always ſpeaks of a galvanic fluid, as ſeveral of Kant's pupils ſpeak of the caloric fluid, the luminous fluid, the electric fluid, and the magnetic fluid. But theſe are mere denominations, which expreſs the unknown cauſes of the phenomena of heat, light, electricity, magnetiſm and galvaniſm. 8th. The vital force, *vita propria*. Blumenbach, to expreſs the phenomena of vitality, ſuppoſes a particular force, which he calls *vita propria*. 9th. *Niſus formativus*. The ſame learned man, to expreſs the formation of organiſed bodies, ſuppoſes a particular force, which he calls *niſus formativus*. This amounts to the ſame as that called by other philoſophers the plaſtic force.

Barthez believes alſo, that we ought to conſider all the vital functions in man and animals, as produced by peculiar forces and directed according to the primordial laws of a vital principle. "I think I have found, adds he, that there exiſts in the living muſcles a force of the fixed ſituation of the moleculæ and of their fibres (tending to reſt) different from the forces of contraction and dilatation (tending to motion), the exiſtence of which has not been before ſo much as ſuſpected. The forces of the vital principle are inherent in each part of the body which they animate, and exerciſe there the movements peculiar to that part. The ſound method of philoſophiſing in each natural ſcience admits in them general *occult cauſes* *.

* Nouvelle Mécanique des Mouvemens de l'Homme, par Barthez.

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As this expreſſion, *occult cauſes*, which modern philoſophy has had ſo much difficulty to baniſh, is inconſiſtent with the generally adopted ideas, I ſhall here offer ſome reflections on that ſubject, as well as on Kant's ſyſtem of forces. In my opinion, the manner of cultivating natural philoſophy ought to be conſidered under ſeveral points of view. 1ſt. Some confine themſelves to a deſcription of the objects and phenomena that occur. The zoologiſt deſcribes animals, their manner of living, and of reproducing their ſpecies. The botaniſt does the ſame in regard to plants. The mineralogiſt in the like manner deſcribes minerals and their external qualities. The phyſical geographer deſcribes the ſurface of the earth; and the aſtronomer the celeſtial bodies, their movements, &c. 2d. Others go ſtill farther. The anatomiſt explores the internal parts of animals and vegetables, and deſcribes their ſtructure; and the chemiſt analyſes them, as well as minerals, to diſcover the principles of which they are formed. 3d. The geometrician calculates the movements and forces of all theſe beings, independently of the cauſes by which they may be produced; and he furniſhes the mechaniſt with calculations for conſtructing and directing his machines. 4th. In the laſt place, others endeavour to diſcover the cauſes of all the phenomena exhibited by theſe bodies, as well as to explain the mechaniſm of their different movements. This is the ſcience of phyſics. But it has ſo often led men into error, that true philoſophers are exceedingly difficult in regard to the explanations it gives. This has induced them to have recourſe to the ſyſtem of forces, by which they expreſs a fact, the effects of which they calculate without attempting to trace them to their cauſes: as they cannot, for example, yet explain the phenomena of vitality, they expreſs them by the words *vital force*, whatever may be their cauſe. The vital force, then, is that which produces the phenomena of vitality; but we are ignorant of the manner in which it acts. This is what ought to be underſtood by the expreſſion *occult cauſes*. The an-

cients, for example, not being acquainted with the weight of the atmoſphere, could not explain why water did not riſe in a pump but to a certain height. They ſaid it was the effect of an occult cauſe; that is to ſay, of a cauſe with which they were unacquainted, which was concealed from them, *occulta*.

In the like manner we ought only to underſtand by the word *force* the cauſe of a general fact, whether this cauſe be known or not. This is the acceptation in which the word was underſtood by Newton. He obſerved that all bodies had a tendency towards each other, and he called the cauſe of this general phenomenon attractive force, or attraction. He obſerved alſo, that in certain circumſtances they repelled each other; and he called the cauſe of this ſecond general phenomenon repulſive force, or repulſion. This mode of expreſſion may be applied to all the grand phenomena of nature, and we may uſe the word force in the following different ſenſes: *Luminous force*, the cauſe of the phenomena of light; the *caloric force*, the cauſe of the phenomena of heat; the *electric force*, the cauſe of the phenomena of electricity; the *magnetic force*, the cauſe of the phenomena of magnetiſm; the *galvanic force*, the cauſe of the phenomena of galvaniſm; the *ſonorous force*, the cauſe of the phenomena of ſound; the *capillary force*, the cauſe of the phenomena of capillary attraction; the *force of affinities*, the cauſe of the phenomena of affinity; the *diſſolving force*, the cauſe of the phenomena of ſolution; the *conſolidating (ſolidifiante) force*, the cauſe of the phenomena of ſolidity; the *cryſtallizing force*, the cauſe of the phenomena of cryſtallization; the *vital force*, the cauſe of the life of animals and vegetables; the *generative force*, the cauſe of the generation of animals and vegetables; the *nutritive force*, the cauſe of nutrition; the *muſcular force*, the cauſe of muſcular motion; the *ſympathetic force*, the cauſe of ſympathetic movements; the *concocting force*, the cauſe which aſſimilates morbific humours. The number of theſe forces might be greatly extended if we

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employ that term to expreſs the cauſe of a general phenomenon.

The geometer calculates the effects of thoſe forces which he employs, as he does algebraic ſigns inſtead of numbers to ſimplify his operations; but if he wiſhes afterwards to obtain reſults, he muſt give to theſe ſigns their real values. In the like manner, if he wiſhes to have a phyſical reſult, he muſt aſſign a value to the word *force* which he employs. The philoſopher endeavours to diſcover the cauſes of theſe forces; but he is ſo often deceived, and gives ſo many falſe explanations, that we are always inclined to conſider as bad thoſe which he aſſigns. We muſt not, however, fall into the oppoſite exceſs, and aſcribe to the word force an acceptation which it ought not to have. Let us imitate the wiſdom of Newton: "All bodies, ſays he, have a tendency towards each other; that tendency I call attraction, attractive force; but it may be the effect of an impulſion, or of ſome other cauſe which is unknown to us." He ſays the ſame thing of repulſive force. Let us apply this to all the forces before mentioned. The ſonorous force, for example, has certainly as its cauſe ſome agitation in the ſonorous body, and the atmoſpheric air by which it is ſurrounded. This is a certain fact, though we cannot yet explain all the phenomena exhibited by ſonorous bodies. The caſe is the ſame with the luminous, electric, magnetic, caloric, and galvanic forces. The phenomena of heat, light, electricity, magnetiſm, galvaniſm, have as cauſes the movements of particular fluids; and, though we are not yet able to determine the nature of all the movements of theſe fluids, we know that their action is in the inverſe ratio of the ſquares of the diſtances.

Reil has ſhewn that the *niſus formativus* of Blumenbach ought to be conſidered, as I have ſaid, as a real force of cryſtallization. In general the formation of organiſed bodies, their increment, their nutrition, is nothing but this force of cryſtallization. It has formed all minerals; the globe itſelf, and the whole univerſe. It is alſo the conſoli-

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dating power, or that which gives ſolidity to all bodies. The force of cryſtalliſation is conſequently a force which acts in the moſt general manner in nature; that is to ſay, among exiſting beings. Attraction itſelf is the effect of the action of the gravific fluid, and repulſion is the effect of the repulſive fluid or caloric. The philoſopher acknowledges that he is yet ignorant of the manner in which all theſe fluids act: he is only acquainted with ſome of their laws.

This acceptation of the word force leaves every poſſible latitude to the calculations of geometry (which form the phyſico-mathematical ſciences), and does not violate the principles of ſound philoſophy. The expreſſion *occult cauſes*, explained in this manner, can give no offence. But if people ſhould perſiſt to conſider force as a quality independent of the action of all matter, fluid or not fluid, this would be a mere metaphyſical abſtraction.

X. *Obſervations on the Satellites of the Georgium Planet, and the two innermoſt Satellites of Saturn. By Dr. J. H.* SCHROTER, *F.R.S. &c. From* Neuere Beytrage zur Erweiterung der Sternkunde, 1798.

I WAS prevented by various ſpeculations, during the hours of obſervation, from directing the thirteen or twenty-ſeven feet reflectors to the Georgium Sidus till the 19th of February 1797, when about eight o'clock in the evening I looked at it through the thirteen feet reflector, with a magnifying power of almoſt 200. It appeared very plainly as a planet of menſurable diameter, having a pale white light; and in a little time, when the eye became accuſtomed to the inſtrument, ſeveral very ſmall ſtars were perceptible around it. Two of theſe, being the neareſt, in *a* and *b* (Plate VI, fig. 1), were a little clearer, though it was with ſome difficulty they could be diſtinguiſhed. The largeſt, *a*, was to the north-

north-weſt, diſtant about ſeven or eight apparent diameters of the planet; but *b*, which was at a much leſs diſtance, was leſs perceptible. They were both too faint for meaſurement. M. Harding diſtinguiſhed them plainly alſo; and both our delineations exactly correſponded. As the planet at an inconſiderable elevation had irradiation, and at the ſame time a large apparent diameter, it was poſſible, according to the diſtance aſſigned by Dr. Herſchel, that they might be the ſatellites; and we conſidered them as ſuch becauſe they had a ſatellite-like appearance.

The weather being ſerene, we were able to obſerve the Georgium planet next evening, the 20th, at eight o'clock, through the ſame reflector with magnifying powers of 180 and 288. It was very evident that we both diſtinguiſhed, with full certainty, the ſame two luminous points, which we had ſeen before, at nearly the ſame diſtance and in almoſt the ſame relative poſitions; ſo that, from their perfectly ſimilar appearance, we had reaſon to conclude they muſt be the ſame ſtars. They had their ſituation in *c* and *d*, in ſuch a manner that *c*, the moſt remote, preceded the planet in the direction of its equatorial motion without perceptible declination.

As uranus, being then retrograde, had within twenty-four hours moved 2′ 40″ towards the weſt, as was viſible from the neighbouring ſtars, the two which we obſerved, had they not been ſatellites, muſt have been ſeen two minutes towards the eaſt. This was not the caſe; but as on the evening before, they were again viſible in *c* and *d*, with the like relative poſition; which fully convinced us that they were ſatellites.

At eight o'clock the following evening we found the ſame luminous points, with the thirteen feet reflector and a magnifying power of 288, at the like diſtance and in the former relative poſition, but more towards the ſouth in *e* and *f*; and it appeared to our conviction, that the ſecond larger ſatellite, which the evening before preceded the planet weſtwards, in

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the direction of its equatorial movement, now formed with it an evident angle, and at the same time had proceeded southwards the like distance. Besides the satellite three telescopic fixed stars were distinguishable in α, β, γ. (fig. 2.)

About ten o'clock we continued our observations with the twenty-seven feet reflector, and M. Harding, besides the small stars which had now moved a little from their place, observed two more in δ and ε (fig. 3.) In the strong light of this telescope the second larger satellite, now moved to *e*, which from its distance, colour, and degree of light, we perceived to be the same observed on the 19th and the 20th, distinguished itself clearly from the other small stars, and even from the first satellite, though its light was very weak. At the same time we observed the direction of its equatorial motion towards ζ, so that both the satellites, according to the proportion of the periods of their revolution, seemed to have moved southwards. By this circumstance, and as it was impossible that with a reflector having so much light we should not, in serene weather, see both the satellites, we were more confirmed in our opinion.

As the weather was unfavourable I could not repeat my observations till the 26th of February, at 42 minutes past 10, when, with the thirteen feet reflector and a magnifier of 288, I distinguished two faint luminous points in quite another position, viz. one in *g* (fig. 1.), which I took to be the second or larger satellite, and another in *b*, which I considered as the first. Both these points corresponded with Herschel's periods; so that we could form no other conclusion than that, with this excellent reflector, we had seen both the satellites.

How far the power of this reflector extends will particularly appear from the following observations on the satellites of Saturn. We had often observed, with this instrument, during clear moon-shine, the five old satellites, and the variation of their light; but I never believed that it

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would ſhew the two innermoſt; and yet I know, with the utmoſt certainty, that this was the caſe.

I. On the 17th of February 1797, after having ſurveyed with it every thing remarkable in regard to Saturn, M. Harding obſerved, about 7 h. 23m. true time, at *a*, (fig. 4.) a difficult to be diſtinguiſhed faint luminous point, which he was inclined to conſider as the ſixth ſatellite. I diſtinguiſhed it alſo; and both our delineations of its poſition coincided ſo far, that it was ſeen, according to its proportional diſtance from the ring, in the orbit of the ſixth ſatellite. Its light, on account of its very ſmall ſize, was ſo weak that it was not always diſtinguiſhable, but only now and then by intervals.

II. About eleven o'clock, the ſame evening, our obſervations were continued; and though it was difficult to find this point again, it was at length diſcovered by us both. About 11 h. 24 m. it appeared to have moved the conſiderable ſegment from *a* to *b*, in the orbit of the ſixth ſatellite. This was ſufficient to convince us that we had obſerved that ſatellite with the thirteen feet reflector and a magnifying power of 288; for in both points its diſtance from the ring, according to the proportion of the ellipſe, amounted, like that of the ſixth ſatellite, to only $\frac{1}{4}$ of the diameter of the ring. 2d. Saturn was retrograde, and, according to calculation, receded weſtwards 3·3 ſeconds, in four hours, from 7 h. 23 m. to 11 h. 24 m. Had it been a fixed ſtar, it muſt have appeared about 11 h. 24 m. nearly 3·3 ſeconds farther from *a* towards the eaſt, inſtead of having moved weſtwards to *b*. It could, therefore, be no fixed ſtar, but a ſatellite. 3d. If the ſegment *a*, *b*, which it appeared to have paſſed over, be reduced to the orbit of the ſixth ſatellite, the point in the courſe of four hours had not completely moved $\frac{1}{3}$ of that orbit; and therefore gives a period of ſomewhat more than 32 hours. The ſynodical period of the ſixth ſatellite, according to the determination of Dr. Herſchel,

Herſchel, amounts to 32 h. 53 m. 9 ſec. A more accurate coincidence could not be expected in ſo ſmall a circle.

III. On the 20th of February, in the evening, as the weather was favourable, we directed the twenty-ſeven feet reflector to Saturn, and M. Harding ſignified that he obſerved, very clearly, a ſmall luminous point, near the ring, which from its diſtance and appearance he thought might be one of the ſatellites. I ſaw it alſo myſelf very diſtinctly and with perfect certainty. Each delineated its poſition ſeparately; and both delineations correſponded exactly. About 10 h. 44 m. true time, he ſaw it, not as exhibited by the front view, but according to its true poſition at *c*, in the orbit of the ſixth ſatellite; and the calculation afterwards made, proved that it actually was the ſixth ſatellite, obſerved in *a* and *b* on the evening of the 17th with the thirteen feet reflector; for from the 17th, 11 h. 24 m., to the 20th, 10 h. 44 m. there had elapſed 71 hours 20 minutes, in which time this ſatellite muſt have performed 2·16, nearly 2 whole periods and not quite $\frac{1}{5}$, of its ſynodical revolution, and have appeared removed not fully one ſixth of its orbit towards the weſt from *b*. If the poſition of the planet at ſuch a weſtern diſtance from *b* be projected, it will be found to correſpond very accurately with the obſerved point *c*.

IV. On the 21ſt of February, when viewing Saturn and his ſatellites, for another purpoſe, with the thirteen feet reflector, I for ſome time found no appearance of the ſixth ſatellite; but I at length diſcovered it, and, as I believed, with perfect certainty. At about 7h. 30m. it ſtood in *b*.

V. About 11 o'clock, the ſame evening, our obſervations were continued with the twenty-ſeven feet reflector and a magnifier of almoſt 200. Deceived by the front view of the inſtrument, I imagined that the ſixth ſatellite, according to the before-made obſervations, muſt ſtand ſomewhat to the right of the ring. I believed alſo that I ſaw a point at *f*, or according to the poſition with the ſmall reflector of the thir-

teen

teen feet teleſcope in *g*; and having deſired M. Harding, who often diſtinguiſhed delicate points better than myſelf, to direct his attention towards that part, he aſſured me that he ſaw very clearly, not only a point in *f*, but alſo a ſecond pale luminous point in *e*, the poſition and diſtance of both which points he delineated very accurately, as they were ſeen with a front view, about 11 h. 34 m. true time; and that at *f* was found to be in the orbit of the firſt, and that at *e* in the orbit of the ſecond inner ſatellite. It was not till the next day that I diſcovered my error occaſioned by the front view; and I found, to our great ſatisfaction, that the luminous point, ſeen at *e* in the orbit of the ſecond inner ſatellite, was actually the ſixth or ſecond inner ſatellite: for, as it was ſeen by a front view in *e*, it muſt have been ſeen by the common mode of obſervation towards the eaſt at *d*. Through miſtake, we by no means ſuſpected the ſixth ſatellite in *e*; and, therefore, the obſervation was proved to be ſo much the more correct when calculation ſhewed that the ſixth ſatellite had actually been, according to the front view, in that poſition, or, according to the uſual mode of obſervation, in *d*. From 10 h. 44 m. on the evening Feb. 20th, till 11 h. 34 m. on the evening of the 21ſt, there had elapſed 24 hours 50 minutes, in which time this ſatellite, according to its period of 32 h. 53 m. 9 ſec. had paſſed over $\frac{3}{4}$ of its period + $\frac{1}{24}$, and, conſequently, muſt have ſtood in *d*; which correſponds perfectly with the point ſeen in *e* by the front view.

I ſhall here take occaſion to remark, that the pale luminous point which M. Harding ſaw, by the front view, at *f*, exactly in the orbit of the ſeventh ſatellite, and of which I myſelf had a tranſient glance, may really have been that ſatellite; for it had often happened before, that, beſides ſeeing the five old ſatellites, I imagined that I ſaw in proportionally near, ſmall, pale luminous points, the two inner, the ſixth and ſeventh ſatellites, when the weather was not ſo favour-

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able as to enable me to examine them particularly for any time without intermission.

VI. At 7 h. 34 m. February 27th, when we were again employed in observing with the thirteen feet reflector, M. Harding assured me that he saw, with perfect certainty, a very faint luminous point at *i*, where I had seen it also a little before, but not with so much certainty. According to his delineation, it stood in the orbit of the sixth satellite; and it was found afterwards by comparison that this luminous point was actually that satellite: for, from 11 h. 34 m. on the evening Feb. 21st, till 7 h. 34 m. on the evening of the 27th, five days 20 hours had elapsed, which divided by the period of its revolution gives 4·25 revolutions; so that the satellite must have stood in *i*, about $\frac{1}{4}$ of its orbit farther from *d* towards the west, where it actually was according to accurate observation.

These six observations, made in the course of ten days, during which the satellite was always seen in those points in which, according to Dr. Herschel's determination, it ought to be found, clearly shew not only that we distinguished the sixth satellite with the thirteen feet reflector, but that the period assigned by Herschel has been determined with great accuracy. Two observations, however, made later appear to me to be worthy of notice.

VII. On the evening of the 6th of March, when Saturn was distant only from $1\frac{1}{4}$ to $1\frac{1}{2}$ degree from the illuminated half of the moon, I discovered the four outer satellites with the thirteen feet reflector and a magnifying power of 288. On account of some measurement which I had long wished to make under such circumstances, I applied to the reflector a magnifying power of 532; and it is certain that with this power, almost double to the former, at so small a distance from the moon, I saw much better, though the light was considerably weakened; for, at the first view, I not only clearly discovered the first satellite in its western digression

and periodical change of light towards faintneſs; but I ſaw alſo to the north-eaſt, at the diſtance from the ring of $\frac{1}{2}$ of its diameter, a very ſmall fixed ſtar, which was only half as perceptible as the pale firſt ſatellite was during its periodical increaſe of light.

M. Harding ſoon after found not only this fixed ſtar with all the five old ſatellites, but obſerved alſo, on the weſtern ſide nearer the ring, a ſtill paler, more delicate luminous point, the ſituation of which he ſhewed only in general. I ſucceeded in diſcovering it alſo; and both our delineations of its poſition being compared, proved that we had both ſeen it very accurately in the ſame point, and in the orbit of the ſixth ſatellite. Even if we had not immediately calculated the place of this ſatellite, we muſt, from the proportional decreaſe of its light and the foregoing obſervations, have conſidered it as the ſixth. About 8 h. 12 m. true time, it ſtood in *k*; and it afterwards appeared that we had not erred; for M. Harding, having continued the obſervation alone, found about 10 h. 30 m. that it had proceeded eaſtwards to *l*. As Saturn at that time moved daily about two minutes towards the eaſt, the luminous point *k*, had it been a fixed ſtar, muſt have been diſtant from Saturn about 10 ſeconds weſtward. This circumſtance, that it not only had accompanied Saturn eaſtwards, but had moved in its orbit, a perceptible portion of a circle, proved with full certainty that it was the ſixth ſatellite, and this was afterwards confirmed by calculation; for from 7 h. 23 m. true time, in the evening, Feb. 17th, when it was firſt ſeen by M. Harding, to 8 h. 12 m. on the 6th of March, 17 days 0 hours 49 minutes had elapſed, during which the ſatellite had completed 12·43 ſynodical revolutions of 1 day 8 hours 53 minutes 9 ſeconds. It muſt therefore have moved about $\frac{43}{100}$, or almoſt the half of its orbit from *a* towards the weſt; and this exactly agrees with the poſition where we both ſaw it in *k*. The arc which it had moved over from *k* to *l* correſponds alſo, becauſe the ſatellite in 2 h. 18 m. from

8 h. 12 m. to 10 h. 30 m. must have moved towards the east about $\frac{1}{14}$th of its orbit, which corresponds with the arc *k l*.

It is also worthy of remark, that we distinguished this satellite with a thirteen feet reflector during serene weather, by moonlight, and only at the distance of $1\frac{1}{4}$ degree from the moon; and the circumstance that, in the course of seventeen days, we found it eight times exactly in those positions where, according to Herschel's period, it ought to stand, confirms in the completest manner the accuracy of that period.

Observations which confirm what others have discovered with large telescopes contain indeed nothing new; but they are, however, still valuable not only to the observer, but to many amateurs, who often wish to try whether they have instruments that possess a sufficiently strong power; and under that view I have considered it as my duty to give the above at full length, selected from a great many which I made in regard to the satellites of Saturn.

I shall pass over several made on the sixth satellite of Saturn, which seem to betray irregularities in it; and only remark that, according to our observations, this sixth satellite, like the whole of Jupiter's four, and the five outermost of Saturn, are, in all probability, subject to a variation of their light.

XI. *Extract of Experiments and Observations on the Use of Phosphorus administered internally. By* Alphonsus Leroi, *Professor at the Medical School of Paris. From* Bulletin de la Societé Philomatique, 1798.

1. Phosphorus administered internally in consumptive diseases appears to give a certain degree of activity to life, and to revive the patients, without raising their pulse in the same proportion. The author relates several instances that

that occurred to him in the courſe of his practice, one of which is as follows: Being called to attend a woman, at the point of death, who was quite worn out by a conſumptive diſorder, with which ſhe had been afflicted for three years, in compliance with the earneſt deſire of her huſband, who requeſted him to give her ſome medicine, he compoſed one of a portion of ſyrup diluted with water in which a few ſticks of phoſphorus had been kept. Next day the woman found herſelf much better. She was revived for a few days; and did not die till about a fortnight after.

2. He himſelf, as he acknowledges, was ſo *imprudent* as to take two or three grains of ſolid phoſphorus combined only with treacle, and experienced the moſt dreadful ſymptoms. At firſt he felt a burning heat in the whole region of the ſtomach. That organ ſeemed to be filled with gas which eſcaped by the mouth. Being dreadfully tormented, he tried to vomit, but in vain, and found relief only by drinking cold water from time to time. His uneaſy ſenſations were at length allayed; but next morning he ſeemed to be endowed with an aſtoniſhing muſcular force, and to be urged with an almoſt irreſiſtible impulſe to try its energy. The effect of this medicine at length ceaſed, adds the author, *à la ſuite d'un priapiſme violent.*

3. In many caſes the author employed, and ſtill employs, phoſphorus internally, with great benefit, to reſtore and revive young perſons exhauſted by exceſſes. He divides the phoſphorus into very ſmall particles, by ſhaking it in a glaſs filled with boiling water. He continues to ſhake the bottle, plunging it into cold water, and thus obtains a kind of precipitate of phoſphorus, exceedingly fine, which he bruiſes ſlowly with a little oil and ſugar, or afterwards employs as liquid electuary, by diluting the whole in the yolk of an egg. By means of this medicine he has effected aſtoniſhing cures, and reſtored the ſtrength of his patients in a very ſhort time.

4. In malignant fevers the uſe of phoſphorus internally, to check the progreſs of gangrene, has ſucceeded beyond expectation. The author relates ſeveral inſtances.

5. Pelletier told him, that having left, through negligence, ſome phoſphorus in a copper baſon, that metal was oxydated, and remained ſuſpended in the water. Having thoughtleſsly thrown out the water in a ſmall court in which ducks were kept, theſe animals drank of it, and all died. *Mais le male*, ſays the author, *couvrit toutes ſes femelles juſque au dernier inſtant de ſa vie.* An obſervation which accords with the effect experienced by the author.

6. The author relates a fact which proves the aſtoniſhing diviſibility of phoſphorus. Having adminiſtered to a patient ſome pills, in the compoſition of which there was not more than a quarter of a grain of phoſphorus, and having had occaſion afterwards to open the body, he found all the internal parts luminous; and even the hands of the perſon who had performed the operation, though waſhed and well dried, retained a phoſphoric ſplendor for a long time after.

7. The phoſphoric acid, employed as lemonade, has been ſerviceable to the author in the cure of a great number of diſeaſes.

8. Leroi aſſures us that he oxydated iron with phoſphorus, and obtained, by the common means, a white oxyd, almoſt irreducible, which he thinks may be employed with advantage in the arts, and particularly in painting with oil, and in enamel, inſtead of the white oxyd of lead. This white oxyd of iron occaſioned violent retchings to the author, who ventured to place a very ſmall particle of it on his tongue. He does not heſitate, therefore, to conſider this oxyd as a terrible poiſon. He was not able to reduce it, but by fixed alkali and the glaſs of phoſphorus.

9. The author aſſerts that by means of phoſphorus he decompoſed and ſeparated from their baſes the ſulphuric, muriatic and nitric acids; that by help of the phoſphoric acid he

he tranſmuted earths; and that with calcareous earth he can make, at pleaſure, conſiderable quantities of magneſia. He declares that to his labours on phoſphorus he is indebted for proceſſes by which he effects the diſſipation (*opère la frite*) of rubies, the fuſion of emeralds, and the vitrification of mercury.

[We need hardly add, that Engliſh practitioners will uſe their wonted caution in the application of ſo powerful a remedy.]

XII. *On the Proceſs of Bleaching with the oxygenated muriatic Acid; and a Deſcription of a new Apparatus for bleaching Cloths with that Acid diſſolved in Water, without the Addition of Alkali. By* THEOPHILUS LEWIS RUPP, *Eſq. From* Memoirs of the Literary and Philoſophical Society of Mancheſter, *Vol. V.* 1798.

FOR the diſcovery of the oxygenated muriatic acid, its effects on colouring matter, and its ineſtimable advantages, the arts are indebted to the juſtly-celebrated Scheele. M. Berthollet loſt no time in applying the properties of this curious and highly intereſting ſubſtance to the moſt important practical uſes. His experiments on bleaching with the oxygenated muriatic acid proved completely ſucceſsful, and he did not delay to communicate his valuable labours to the public. The new method of bleaching was quickly and ſucceſsfully introduced into the manufactures of Mancheſter, Glaſgow, Rouen, Valenciennes, and Courtray; and it has ſince been generally adopted in Great Britain, Ireland, France, and Germany. The advantages which reſult from this method, which accelerates the proceſs of whitening cottons, linens, paper, &c. to a really ſurpriſing degree, in every ſeaſon of the year, can be juſtly appreciated by commercial people only, who experience its beneficial effects in

many ways, but particularly in the quick circulation of their capitals.

Great difficulties, for a time, impeded its progreſs, ariſing chiefly from prejudice and the ignorance of bleachers in chemical proceſſes. Theſe obſtacles were, however, ſoon removed, by Mr. Watt at Glaſgow, and by Mr. Henry and Mr. Cooper at Mancheſter. Another difficulty preſented itſelf, which had nearly proved fatal to the ſucceſs of the operation. This was the want of a proper apparatus, not for making the acid and combining it with water, for this had been ſupplied in a very ingenious manner by Mr. Watt and M. Berthollet*; but for the purpoſe of immerſing and bleaching goods in the liquor. The volatility of this acid and its ſuffocating vapours prevented its application in the way commonly uſed in dye-houſes. Large ciſterns were therefore conſtructed, in which pieces of ſtuff were ſtratified; and the liquor being poured on them, the ciſterns were cloſed with lids. But this method was ſoon found to be defective, as the liquor could not be equally diffuſed: the

* M. Berthollet's apparatus, however, is too complex for the uſe of a manufactory; Mr. Watt's is better; but a range of four, five, or ſix hogſheads, or rum-puncheons, connected with one another, in the manner of Woulfe's diſtilling apparatus, is preferable to either of them. Agitators, on M. Berthollet's principle, may be applied. The retort or matraſs ſhould be of lead, ſtanding in a water-bath; its neck ſhould be of ſufficient length to condenſe the common muriatic acid, which always comes over, and it ſhould form an inclination towards the body of the retort, ſo that the condenſed acid may return into it. I beg leave to obſerve here, that I always found the liquor to be ſtrongeſt when the diſtillation was carried on very ſlowly. I have alſo found, that the ſtrength of the liquor is much increaſed by diluting the vitriolic acid more than is uſually done. The following proportions afforded the ſtrongeſt liquor:

Three parts manganeſe.
Eight parts common ſalt.
Six parts oil of vitriol.
Twelve parts water.

The proportion of manganeſe is ſubject to variation according to its quality.

pieces

pieces were, therefore, only partially bleached, being white in ſome parts and more or leſs coloured in others. Various other contrivances were tried without ſucceſs, till it was diſcovered that an addition of alkali to the liquor deprived it of its ſuffocating effects, without deſtroying its bleaching powers. The proceſs began then to be carried on in open veſſels, and has been continued in this manner to the preſent period. The bleacher is now able to work his pieces in the liquor, and to expoſe every part of them to its action, without inconvenience. This advantage is unqueſtionably great; but it is diminiſhed by the heavy expence of the alkali, which is entirely loſt. It is moreover to be feared, that the alkali which is added to the liquor, though it does not deſtroy its power of bleaching, may diminiſh it; becauſe a ſolution of the oxygenated muriat of potaſh, which differs from the alkaline bleaching liquor in nothing but in the proportion of alkali, will not bleach at all. This is a well known fact, from which we might infer, that the oxygenated muriatic acid will loſe its power of deſtroying the colouring matter of vegetable ſubſtances, in proportion as it becomes neutralized by an alkali. But as we ſhould not content ourſelves with inferences however plauſible, when the truth may be eſtabliſhed by experiment; and as I thought the matter of ſufficient importance, I made the following experiments on the ſubject.

I beg leave to premiſe, that in all theſe experiments I made uſe of one and the ſame acid, which was kept in a bottle with a ground-glaſs ſtopper, and ſecured from the influence of light. The manner in which I made the experiments was ſimply this. I weighed, firſt of all, a bottle filled with the colouring ſubſtance which I meant to employ: I then weighed, in a large and perfectly colourleſs bottle, half an ounce of the acid, to which I immediately, but very gradually, added of the colouring ſubſtance contained in the former bottle, till the acid ceaſed to deſtroy any more of its colour. The bottle with the colouring ſubſtance

was then weighed again, and the difference between its present and original weight was noted. The same method was observed in all the experiments.

Experiment I. To half an ounce of oxygenated muriatic acid, I added a solution of indigo in acetous acid*, drop by drop, till the oxygenated acid ceased to destroy any more colour. It destroyed the colour of 160 grains of the acetite of indigo.

Experiment II. A repetition of Experiment I. The colour of 165 grains of acetite of indigo was destroyed in this experiment.

Experiment III. A repetition of Experiments I. & II. The colour of 160 grains of the acetite was destroyed.

Experiment IV. To half an ounce of the oxygenated muriatic acid, were added 8 drops of pure potash in a liquid state. This quantity of alkali was about sufficient to deprive the acid of its noxious odour. This mixture destroyed the colour of 150 grains of the acetite of indigo.

Experiment V. A repetition of Experiment IV. The colour of 145 grains of the acetite was destroyed.

Experiment VI. To half an ounce of the oxygenated muriatic acid, 10 drops of the same alkali were added. It destroyed the colour of 125 grains of the acetite of indigo.

Experiment VII. A mixture of half an ounce of the oxygenated acid, and 15 drops of the alkali, destroyed the colour of 120 grains of the acetite of indigo.

Though I had taken the precaution of avoiding the sulphuric acid, for the reason stated in the foregoing note, I was not quite satisfied with these experiments, on account

* It has been usual to estimate the strength of the oxygenated muriatic acid by a solution of indigo in sulphuric acid. This method was inadmissible in these experiments on the comparative strength of the bleaching liquor, with and without alkali; because the sulphuric acid would have decomposed the muriat of potash, and thereby produced errors. I therefore added to a solution of indigo in sulphuric acid, after it had been diluted with water, acetite of lead, till the sulphuric acid was precipitated with the lead. The indigo remained dissolved in the acetous acid.

of

of errors which might have taken place through a double affinity. I therefore made the following experiments, in which I employed a decoction of cochineal in water, inſtead of the acetite of indigo.

Experiment VIII. To half an ounce of the oxygenated muriatic acid, a decoction of cochineal was added till the acid ceaſed to act on its colour. It deſtroyed the colour of 390 grains of the decoction.

Experiment IX. A repetition of Experiment VIII. The colour of 385 grains of the decoction was deſtroyed in this experiment.

Experiment X. To half an ounce of the acid, 6 drops of the liquid alkali were added. This mixture deſtroyed the colour of 315 grains of the decoction.

Experiment XI. Eight drops of the alkali were mixed with half an ounce of the acid. This mixture deſtroyed the colour of 305 grains of the decoction.

On a comparative view of the reſults of theſe experiments, it will appear, that an addition of potaſh to the bleaching liquor impairs its ſtrength conſiderably. This diminution of power, and the expence of potaſh, are a ſerious loſs in an extenſive manufacture. It would, therefore, be deſirable to have an apparatus for the uſe of the pure oxygenated muriatic acid ſimply diſſolved in water, which is at once the cheapeſt and beſt vehicle for it. This apparatus muſt be ſimple in its conſtruction, and obtainable at a moderate expence; it muſt confine the liquor in ſuch a manner as to prevent the eſcape of the oxygenated muriatic acid gas, which is not only a loſs of power, but alſo an inconvenience to the workmen and dangerous to their health; and it muſt, at the ſame time, be ſo contrived, that every part of the ſtuff which is confined in it, ſhall certainly and neceſſarily be expoſed to the action of the liquor in regular ſucceſſion. Having invented an apparatus capable of fulfilling all theſe conditions, I have the pleaſure of ſubmitting a deſcription of it to the Society, by means of the annexed drawing.

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DESCRIPTION OF THE APPARATUS.

Fig. 1, (Plate VII.) is a ſection of the apparatus. It conſiſts of an oblong deal ciſtern ABCD, made water-tight. A rib, EE, of aſh or beech wood, is firmly fixed to the middle of the bottom CD, being mortiſed into the ends of the ciſtern. This rib is provided with holes, at FF, in which two perpendicular axes are to turn. The lid, AB, has a rim, GG, which ſinks and fits into the ciſtern. Two tubes, HH, are fixed into the lid, their centres being perpendicularly over the centres of the ſockets, FF, when the lid is upon the ciſtern. At I, is a tube by which the liquor is introduced into the apparatus. As it is neceſſary that the ſpace within the rim, GG, be air-tight, its joints to the lid, and the joints of the tubes, muſt be very cloſe; and, if neceſſary, ſecured with pitch. Two perpendicular axes, KL, made of aſh or beech wood, paſs through the tubes, HH, and reſt in the ſockets, FF. A piece of ſtrong canvas, M, is ſewed very tight round the axis K, one end of it projecting from the axis. The other axis is provided with a ſimilar piece of canvas. N, are pieces of cloth rolled upon the axis L. Two plain pulleys, OO, are fixed to the axes, in order to prevent the cloth from ſlipping down. The ſhafts are turned by a moveable handle, P. Q, a moveable pulley, round which paſſes the cord, R. This cord, which is faſtened on the oppoſite ſide of the lid (ſee fig. 2), and paſſes over the ſmall pulley S, produces friction by means of the weight T. By the ſpigot and fauſſet V, the liquor is let off, when exhauſted.

Fig. 2. A plan of the apparatus, with the lid taken off.

THE MANNER OF USING THE APPARATUS.

The dimenſions of this apparatus are calculated for the purpoſe of bleaching twelve or fifteen pieces of $\frac{4}{4}$ calicoes, or any other ſtuffs of equal breadth and ſubſtance. When the goods are ready for bleaching, the axis, L, is placed on a frame

frame in a horizontal position, and one of the pieces, N, being fastened to the canvas, M, by means of wooden skewers, in the manner represented in fig. 1, it is rolled upon the axis by turning it with the handle, P. This operation must be performed by two persons; the one turning the axis and the other directing the piece, which must be rolled on very tight and very even. When the first piece is on the axis, the next piece is fastened to the end of it by skewers, and wound on in the same manner as the first. The same method is pursued till all the pieces are wound upon the axis. The end of the last piece is then fastened to the canvas of the axis K. Both axes are afterwards placed into the cistern, with their ends in the sockets F F, and the lid is put on the cistern by passing the axes through the tubes H H. The handle P is put upon the empty axis, and the pulley Q upon the axis on which the cloth is rolled, and the cord R, with the weight T, is put round it and over the pulley S. The use of the friction, produced by this weight, is to make the cloth wind tight upon the other axis. But as the effect of the weight will increase as one cylinder increases and the other lessens, I recommend that three or four weights be suspended on the cord, which may be taken off gradually, as the person who works the machine may find it convenient. As the weights hang in open hooks, which are fastened to the cord, it will be little or no trouble to put them on and to remove them.

Things being thus disposed, the bleaching liquor is to be transferred from the vessels in which it has been prepared into the apparatus, by a moveable tube passing through the tube I, and descending to the bottom of the cistern. This tube being connected with the vessels, by means of leaden or wooden pipes provided with cocks, hardly any vapours will escape in the transfer. When the apparatus is filled up to the line *a*, the moveable tube is to be withdrawn, and the tube I closed. As the liquor rises above the edge of the rim G, and above the tubes H H, it is evident that no evapora-

tion

tion can take place, except where the rim does not apply cloſely to the ſides of the box; which will, however, form a very trifling ſurface if the carpenter's work be decently done. The cloth is now to be wound from the axis L upon the axis K, by turning this; and when this is accompliſhed, the handle P and pulley Q are to be changed, and the cloth is to be wound back upon the axis L. This operation is, of courſe, to be repeated as often as neceſſary. It is plain, that by this proceſs of winding the cloth from one axis upon the other, every part of it is expoſed, in the moſt complete manner, to the action of the liquor in which it is immerſed. It will be neceſſary to turn, at firſt, very briſkly, not only becauſe the liquor is then the ſtrongeſt, but alſo becauſe it requires a number of revolutions, when the axis is bare, to move a certain length of cloth in a given time, though this may be performed by a ſingle revolution when the axis is filled. Experience muſt teach how long the goods are to be worked; nor can any rule be given reſpecting the quantity and ſtrength of the liquor, in order to bleach a certain number of pieces. An intelligent workman will ſoon attain a ſufficient knowledge of theſe points. It is hardly neceſſary to obſerve, that, if the liquor ſhould retain any ſtrength after a ſet of pieces are bleached with it, it may again be employed for another ſet.

With a few alterations, this apparatus might be made applicable to the bleaching of yarn. If, for inſtance, the pulley O were removed from the end of the axis K, and fixed immediately under the tube H;—if it were perforated in all directions, and tapes or ſtrings paſſed through the holes, ſkains of yarn might be tied to theſe tapes underneath the pulley, ſo as to hang down towards the bottom of the box. The apparatus being afterwards filled with bleaching liquor, and the axis turned, the motion would cauſe every thread to be acted upon by the liquor. Several axes might thus be turned in the ſame box, and being connected with each other by pulleys, they might all be worked by one perſon at the

ſame

ſame time; and as all would turn the ſame way and with the ſame ſpeed, the ſkains could not poſſibly entangle each other.

In order to ſhew the uſefulneſs of this apparatus ſtill more clearly, I requeſt the Society to attend to the following ſtatement of the expence of a given quantity of bleaching liquor, with and without alkali, but of equal ſtrength.

WITH ALKALI*.

	£.	s.	d.
80 lb. of ſalt, at 1½d. per lb. - - -		10	0
60 lb. of oil of vitriol, at 6½d. per lb. - -	1	12	6
30 lb. of manganeſe - - -		2	6
20 lb. of pearl-aſhes, at 6 d. per pound - -		10	0
	£2	15	0

But it appears by the foregoing experiments, that the liquor loſes ſtrength by an addition of alkali. The value of this loſs, which on an average amounts to 15 per cent. muſt be added to the expence - - - - - 8 3

£3 3 3

WITHOUT ALKALI.

80 lb. of ſalt - - - -		10	0
60 lb. of oil of vitriol - - -	1	12	6
30 lb. of manganeſe - - -		2	6
	£2	5	0

It appears from this calculation, that a certain quantity of the liquor, for the uſe of my apparatus, coſts only 2l. 5s. but that the ſame quantity of the alkaline liquor coſts 3l. 3s. 3d. which is 40 per cent. more than the other. The aggregate of ſo conſiderable a ſaving muſt form a large ſum in the extenſive manufactures of this country.

* I make no mention of the expence attending the *preparation* of the liquor, it being the ſame in both caſes.

XIII. *A Diſſertation on Painting in Oil in a Manner ſimilar to that practiſed in the Ancient Venetian School. By Mr.* TIMOTHY SHELDRAKE *. *From* Tranſactions of the Society for the Encouragement of Arts, *Vol. XVI.* 1798.

THE method of painting practiſed in the Venetian School, I conceive to have been as follows:

The cloth was primed with colours in diſtemper, of a browniſh hue, ſuch as would properly enter into the darkeſt parts of the picture. The moſt tranſparent colours are the propereſt. I believe umber was moſt generally uſed, broken with red, yellow, or blue, according to the tint intended to be produced, and diluted with chalk or whiting to the proper degree of ſtrength. Upon the ground ſo prepared the ſubject was correctly drawn with umber, pure, or mixed with lake, blue, or black; and, with the ſame colours, thoſe ſhadows that were darker than the ground were then painted in.

The artiſt then painted the lights with *pure white*, in a ſolid body, where the light was brighteſt, or where the full effect of colour was to be produced; and, where the demi-tints were afterwards to be, ſcumbling it thinner by degrees, till it united with the ſhadows.

In this manner the chiaro-oſcuro was finiſhed as much as poſſible, and the local colour of every object in the picture glazed over it. All the colours uſed in this part of the work were ground in oil, which was abſorbed into the ground, the picture remaining flat, ſomething like a picture in water colours or crayons; it was then varniſhed, till ſaturated with varniſh, and the full of every colour brought out: the picture was then complete.

* The Great Silver Pallet was voted to Mr. Sheldrake for this ingenious performance.

Upon

Upon the moſt ſuperficial view of this proceſs, it will be evident that a picture painted by it is, as to all viſible properties, a varniſh picture; for the ſmall quantity of oil that had been uſed, ſunk into the ground, and never could riſe again to be hurtful; while the varniſh, being laid on after the colours, gave them all the brilliancy and durability they could derive from that vehicle, without being liable to the objections that are made to painting in varniſh, ſuppoſing it to be uſed in the ſame manner as oil is in painting with oil. It is true that this mode of painting is itſelf liable to ſome objections: theſe I have endeavoured to obviate, and ſhall therefore mention hereafter. Here it may be obſerved that, as any varniſh may be uſed, it is to this circumſtance we muſt attribute the different degrees of durability in pictures of the Venetian School. I have ſeen ſome that would reſiſt the moſt powerful ſolvents, while others were deſtroyed by the weakeſt; though all poſſeſſed the apparent properties that diſtinguiſh the Venetian pictures from all others.

As I do not pretend to degrade painting to the rank of a mechanical art, that may be infallibly practiſed by a receipt, I ſhall be permitted to obſerve, that this was the general ſyſtem of the Venetian School, which I have ſeen variouſly modified in the works of different artiſts of that, as well as of the Flemiſh, which is derived from it. It is ſuſceptible of an almoſt infinite number of modifications, in proportion to the talents, the judgment of, and the objects to be painted by, the artiſts who may adopt it. This being the caſe, if it is proved by experiment that effects ſimilar to thoſe of the Venetian pictures may be produced by this method, and that the ſyſtem has a ſtrong tendency to produce that brilliancy, and harmony of colouring that is ſo much admired, with more certainty and facility than thoſe qualities can be obtained by any other mode of painting, I hope my caſe will be proved to the ſatisfaction of the Society.

I once aſked Sir Joſhua Reynolds, by what circumſtances in the management of a picture he thought the harmony of colouring

colouring was to be produced? He replied, An unity of light and an unity of ſhadow ſhould pervade the whole. He explained to me the difficulty of reducing the various colours of all the objects that may be included in a picture, and the various modifications of thoſe colours to the ſimple, harmonious ſtate he deſcribed, and illuſtrated what he had ſaid by this ſimile. "A picture, to poſſeſs harmony of colouring, ſhould look as if it was painted with one colour (ſuppoſe umber and white), and, when the chiaro-oſcuro was complete, the colour of each object ſhould be glazed over it."

This obſervation, from ſuch authority, was impreſſed with peculiar force on my mind; and if I can retrace its operations on a ſubject which has ſo long engaged my attention, I ſhould ſay Sir Joſhua's obſervation was the clue that guided me through all my experiments, and, I hope, will enable me to prove, that the beautiful and ſimple practice which he ſuggeſted as a ſimile, was literally the practice of that ſchool upon whoſe works his ideas of colouring were founded. At the ſame time I may obſerve, that the fact ſeems to have eluded his obſervation, or he would not have uſed it as a compariſon to ſimplify his deſcription of a practice which he thought both difficult and complex.

In the Newtonian doctrine of light and colours, it is believed that all colours are inherent in light, and are rendered viſible by the action of various bodies, which reflect particular rays, and abſorb the reſt. Without diſputing the truth of this doctrine, it is to be obſerved, that a painter muſt conſider the objects he repreſents as being analogous to the materials he uſes to repreſent them; and, in this view of the ſubject, colour is to be conſidered as a property inherent in bodies, which is rendered viſible by the contact of light, a colourleſs, or at leaſt a mono-coloured ſubſtance, and ſhadow the mere privation of light.

A picture may repreſent either a group of figures, or other objects, in a room, or any objects in the open air: whatever the ſituation may be, it repreſents certain objects in a given

ſpace, poſſeſſing individually their peculiar colours, and generally expoſed to the operations of light. The quantity of light each can receive muſt depend upon its form, and its poſition reſpecting that part whence the light comes; for, in proportion as other parts recede from the light, the ſhadow becomes viſible: but ſhadow is nothing but privation of light, and privation of colour, in proportion as the light is diminiſhed. Some attention to theſe circumſtances will, perhaps, enable us to demonſtrate the truth of Sir Joſhua's poſition.

If a globe of one colour be expoſed in a painter's room, properly darkened, that part which is neareſt the light will partake of its colour; the next part will ſhew the true colour of the object: that which firſt recedes from the light will be a little obſcured, the next a little more, and ſo on progreſſively, till that part which is fartheſt from the light will loſe its colour, and appear equally dark with the ſhadieſt part of the room. Now we know this globe is of one uniform colour; the variations we ſee in different parts of it are only deceptions, occaſioned by the acceſſion of light in ſome parts, and the privation of it in others.

What is true of this one object and its parts, would be equally true of any number of objects, whatever their colours or relative ſituations might be: if they were placed together in the ſame room, each would poſſeſs its own individual colour, each would partake of the general light, in proportion to its ſituation, and of the general darkneſs in proportion as it recedes from the light. All this may be eaſily conceived; but the difficulty, and in the ordinary modes of painting a ſerious one it is, is to repreſent ſuch objects with the appearance of truth, and preſerve the harmony neceſſary to conſtitute a whole. The Venetian painters however, by whatever means they obtained their knowledge, diſcovered a method ſo ſimple, that perhaps no other can produce ſuch brilliant effects, and undoubtedly not with facility and certainty at all comparable with theirs.

The artiſt will remark that, in deſcribing the whole of the Venetian method of painting, I have ſaid nothing of the manner of produeing thoſe demi-tints which conduce ſo much to the brilliancy of a picture, which are ſo difficult to execute, and in which he moſt frequently fails. Thoſe tints are, in the ordinary modes of painting, produced by the mixture of black, grey, blue, or brown (according to the judgment of the artiſt), with the local colours of the objects. It is theſe tints which, from their being made with ſuch colours, it is difficult to get clear, and which never are ſo clear in any other as in the Venetian, and in ſome of the Flemiſh pictures, which are painted upon analogous principles. The fact is, that thoſe painters produced all ſuch tints without the admixture of any colour to repreſent them, and by a method ſo like that by which they are produced in nature, that this circumſtance alone enſures a degree of brightneſs to their colours, and of harmony to their ſhadows, that it is perhaps impoſſible to produce, in an equal degree, by any other mode of painting.

It is a ſingular fact, which I have not ſkill in phyſics to be able to account for, though by numerous experiments I have aſcertained beyond contradiction, that if upon any degree of brown, between the deepeſt and the lighteſt brown yellow, we paint pure white, in gradations, from the ſolid body to the lighteſt tint that can be laid on, *all the tints between the ſolid white and the ground will appear to be* GREY, intenſe in proportion to the depth of the ground, and the thinneſs of the white laid upon it. But in every caſe all the tints laid upon one ground will harmonize with each other, and form one connected chain (if I may uſe the expreſſion), which will perfectly unite the higheſt light with the darkeſt ſhade.

If then we examine the component ſubſtances of a Venetian picture, we ſhall find the lighter parts conſiſt only of white, to repreſent the light; and of the local colours of the objects it repreſents, the demi-tints are imitated by an *ap-*

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pearance almoſt as deceptive as the ſimilar appearances in nature: but in every other method of painting, theſe demi-tints are produced by mixing ſome duſky colour with the local colours and the light. The compariſon of theſe methods will afford a demonſtrative reaſon why the Venetian muſt be brighter than any other mode of painting.

Having ſhewn, as near to a demonſtration as the nature of the ſubject will perhaps admit, why thoſe parts of a Venetian picture that are connected with light and colours are brighter than the correſponding parts of any other pictures, it remains to explain the cauſe of ſimilar ſuperiority in the darker parts of the ſame pictures.

It has been ſaid, with much confidence, that as white repreſents light, ſo black is the repreſentative of darkneſs. But though this may be true in phyſics, it certainly is not ſo in painting: for the painter's art is to repreſent objects as they *appear*, in point of colour, to be, not as they really are. Thus, if I know an object is perfectly black, and am to repreſent it as it appears to be at the diſtance of fifty feet, black from the pallet will not produce a good imitation of it, becauſe the interpoſition of fifty feet of the atmoſphere will cauſe it to appear of a colour different from what it really is; and *vice verſa*, if we go into a cavern, a cellar, or a room, ſo darkened that the colour of no object can be diſtinctly ſeen, and if we there hold any ſolid black ſubſtance near to the eye, the difference will be viſible at once; the black object will be immediately diſtinguiſhed, by its ſolidity and colour, from the ſurrounding ſpace, and ſuch remote objects as may be obſcurely viſible through it. Theſe objects actually poſſeſs their individual colours, and only appear indiſtinctly from the abſence of light. The black object may appear ſolid, and of that colour, from its proximity to the eye; but the circumjacent ones will appear of a colour perfectly diſtinct from it, more or leſs tranſparent, in proportion to their diſtance from the eye, and ſhewing a portion of their individual colours, according to

the quantity of ill-defined light that may be admitted. Thus we ſee (if I may venture to mention ſo notorious a truiſm), that ſhadows are nothing real; they only ſeem to exiſt in the abſence of light, and give to objects an ill-defined appearance, diſtinct from, though in ſome inſtances mixed with, light and colours in different degrees: but as the painter muſt repreſent this *appearance* by ſomething *real*, he chooſes the colours moſt analogous, viz. browns, and the moſt tranſparent of their claſs, to repreſent this tranſparent, but imperfectly defined appearance in nature.

It has been ſuppoſed that the Venetian painters had ſome peculiarly rich and tranſparent brown colour, which is ſeen to pervade all the works of that ſchool; the effect of which no modern artiſt has been able to imitate, and which therefore is ſuppoſed to have been loſt. It is not very probable that a colour ſo common, as to pervade the works of the worſt as well as of the beſt artiſts of that ſchool, ſhould be ſo unaccountably loſt; and, as the effect attributed to it may be eaſily produced by the mode of painting I have deſcribed, it is not unreaſonable to conclude that this much-lamented colour has never exiſted.

It is well known that chalk, and other earths of the ſame kind, loſe, when wetted, much of their whiteneſs, and become ſemi-tranſparent: it is equally certain, that if umber or other earths are mixed with chalk, and ſaturated with varniſh after they are laid in the cloth, they in like manner become diaphanous, and are infinitely more brilliant than the ſame colours can be when mixed with white lead and oil. This ſeems, on good grounds, to have been the baſis of the Venetian method of painting, and all its peculiar effects; at leaſt if I may draw any concluſion from the numerous experiments I have made. But if artiſts, whoſe talents will enable them to repeat thoſe experiments to the beſt advantage, ſhould be induced to do ſo, the fact will be determined in the moſt ſatisfactory manner.

I may now be permitted to ſay, it is difficult, if not impoſſible, to conceive a theory more ſimple, more beautiful, or more true, than that of Sir Joſhua Reynolds. It is certainly impoſſible to form a practice more ſimple, or more conformable to that theory, than the one I have deſcribed, as will be evident on recapitulating the particulars.

The artiſt, having determined what hue ſhould pervade his picture, formed his ground with that colour prepared in diſtemper: upon this the ſubject was drawn, and the darker ſhades painted in with tranſparent colours, which ſunk into the ground: with pure white he then painted in all the lights and demi-tints, and, laſtly, glazed in the colours, each in its place. Upon applying the varniſh, the darker ſhades were, as to body, incorporated with the ground; and thus, though different in colour, appeared thinner and more tranſparent than any colours could be when laid upon any ground: the full effect of every colour was brought out, and the picture was complete.

Whoever has been accuſtomed to paint, or to mark the progreſs of painting in the common way, and will reflect on the practice of the method I have deſcribed, by artiſts who had been brought up to it, muſt ſee that ſuch artiſts would paint with a degree of facility, expedition, and certainty, as to effect, that could never be equalled in the ordinary way of painting in oil: beſides, it will be evident that an artiſt would not only paint a picture himſelf with more facility, but, if he had occaſion, could employ a number of ſubordinate artiſts upon large works, and put thoſe works out of hand with more uniformity, as to merit and effect, than if he were to employ ſuch aſſiſtants in ſimilar works if they were to be painted in the common way.

I am ſenſible how little attention will, and perhaps ought to be paid to obſervations on painting, if made by thoſe who are not profeſſionally artiſts: for practical men acquire a kind of knowledge that can never be obtained in any other way; but at the ſame time they contract prejudices that

often prevent them from fully inveſtigating any novelty in practice that may be offered to their notice. The ſpeculative man, on the contrary, who inveſtigates the properties of matter, unſhackled by practical prejudices, and with ideas purely chemical or philoſophical, will be more likely to aſcertain all the facts relative to any theory that may become the object of enquiry. In this way I hope I have proceeded in this inveſtigation. I have endeavoured to conſider pictures as maſſes of matter, poſſeſſing the properties, but differing from each other in degree of brilliancy, tranſparency, and duration. I have endeavoured to aſcertain the cauſes of this difference, with what ſucceſs the Society will determine, and with what utility muſt be hereafter aſcertained by the practice of thoſe for whoſe uſe the inveſtigation was intended.

I ought, perhaps, here to take leave of the ſubject; but as I have been induced to ſubmit to the Society's notice an attempt at painting, it may be expected that I ſhould give an account of the manner in which the pictures I have ſent were painted.

I have already mentioned, that there are ſome difficulties in the method of painting I have deſcribed, as being that of the old artiſts, and which would form objections of conſiderable force to the practice of it by artiſts who are well acquainted with the uſual modes of painting: theſe difficulties are, firſt, the ground abſorbs the oil from the colours ſo faſt, that they are not ſo manageable as in oil-painting; ſecondly, the effect of the picture is not ſeen till the finiſhing varniſh is laid on; and thirdly, as the effect is not ſeen till the picture is finiſhed, it will ſometimes diſappoint the artiſt, and in that caſe it will be difficult, if at all practicable, to alter it.

As I believe the proceſs I have deſcribed in the beginning of this paper is ſimilar to that of Miſs Provis, the artiſts who are acquainted with her recipe can aſcertain whether my conjectures on this ſubject are right or not. I am certain

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at leaſt that theſe difficulties occurred in my attempts to paint, and to obviate them I adopted the following proceſs.

I prepared the ground in diſtemper, and painted the dark parts in the way I have deſcribed; I then varniſhed the ground with the copal oil-varniſh, till it was fully ſaturated, and by this means the full effect of that part of the picture was ſeen: upon this I painted the lighter parts with white, uſing much of the vehicle where the colour was thin, and little in the ſolid parts, leaving the white in them dead: by this means I underſtood the effect of my chiaro-oſcuro, as I ſaw the effect of the demi-tints nearly as well as when the picture was finiſhed.

Upon this I glazed the colours, in the way I have deſcribed in the beginning of this paper, and finiſhed the picture. I often found (probably from want of practice) that the effect was different from what I intended, as the effect of the colours, added to that of the chiaro-oſcuro, produced an eſſential alteration in the whole: as I took care in general that the defect did not ariſe from too much white, I added more where it was deficient, and glazed freſh colours over it, which united perfectly with what had been done before, and did not give the appearance of a mended picture. If the defect was from too much white, I glazed on it a colour ſimilar to that of the ground, painted with freſh white upon that, and glazed the proper colours over it. In this way I found I could alter the picture, but not ſo well as in the former caſe.

I found I could, when neceſſary, increaſe the effect of the picture, by painting on the principal maſs or maſſes of light with the local colours, only mixed with white; as this practice brought thoſe parts more forward, by making them appear ſolid, and thus contraſting them with the tranſparency of the reſt of the picture. In this way I found I could uſe the colours tempered with copal varniſh without difficulty; and I believe, that if I had been accuſtomed to paint large

pictures, I could have painted one as large as life, as eaſily as either of thoſe I have ſubmitted to the Society's inſpection.

XIV. *Some Account of the Travels of the two French Naturaliſts* BRUGUIERE *and* OLIVIER. *From* La Decade Philoſophique, *No. VII.* 1798.

IN the year 1793, during the miniſtry of Roland, Citizens Bruguiere and Olivier, the firſt known by a work upon ſhells and reſearches reſpecting microſcopic animals, and the ſecond by a hiſtory of inſects, were ſent at the expence of government to the eaſtern parts of Aſia. They were to traverſe the Archipelago, Greece, Turkey, Perſia, &c. for the purpoſe of making diſcoveries in natural hiſtory, and of collecting facts and obſervations reſpecting that ſcience. Their miſſion extended to the arts and the ſciences in general; and they were inſtructed to procure every information they could in regard to the agriculture and political economy of the countries through which they might travel. Soon after their departure, however, by the unſettled ſtate of the French government, they were left deſtitute of its aſſiſtance; but their love for natural hiſtory ſupported them under this misfortune. They continued their journey, made valuable collections while traverſing deſerts and mountains, ſtudied the manners of the people, and endeavoured as far as lay in their power to gratify the wiſhes of naturaliſts, and of thoſe fond of the arts and ſciences. After five years labour they were at length on their return home, and had happily arrived at Ancona, when C. Bruguiere was ſnatched by death from his friends and the rewards which awaited him in France. This loſs is announced by C. Olivier to C. Thouin, profeſſor at the muſeum of natural hiſtory, in the following letter, which contains alſo ſome details reſpecting the ſucceſs of a tour which muſt be intereſting to the public.

Genoa,

Genoa, Brumaire 8th, Year VII.

CITIZEN,

Cit. Bruguiere, after having eſcaped the dangers of the ſea, and being almoſt recovered from his fatigue, fell ill at Ancona on the very day of our arrival; and I had the misfortue to loſe a friend, the companion of my travels, on the eleventh day of his illneſs.

I can hardly find words to expreſs with what painful ſenſations I was affected by this unexpected death.

Cit. Bruguiere had kept his health during the courſe of our travels through the iſlands of the Archipelago, and even in Egypt, becauſe the principal part of them had been performed at ſea, and becauſe I alone undertook all that part which required a greater degree of fatigue; but when we quitted the coaſts of Syria to penetrate farther into the country, and when we joined a caravan, which rendered it neceſſary for us to travel thirty or forty days ſucceſſively without reſting, C. Bruguiere's health became ſo deranged, that ſeveral months repoſe at the court of Perſia was not ſufficient to re-eſtabliſh it. After ſtaying, however, ſix months at Bagdad, Bruguiere found himſelf in a condition to traverſe the deſert; and though the ſeaſon was already very hot, our journey was exceedingly pleaſant, on account of the precaution which he took to travel in a kind of carriage. As for me, I continued to ride on horſeback, becauſe I could readily diſmount in order to collect plants, ſeeds, and other objects, and could again ſoon rejoin the caravan; to loſe ſight of which was often attended with danger.

Fatigue, however, was not the only cauſe of the continual illneſs of Bruguiere while in Perſia. The want of wine contributed greatly towards it on many occaſions, eſpecially as the water in general is brackiſh, and of a purgative quality. The chemiſts will eaſily give an explanation of this circumſtance, when I have communicated to them the curious obſervations I have made on that ſubject. I held out better

than

than Bruguiere, becauſe I was younger, more habituated to bodily exerciſe, and accuſtomed to drink water.

We ſpent ſome time on the unhealthful coaſt of Syria, waiting for a veſſel to convey us to Europe. We even traverſed the iſland of Cyprus without any accident, during the moſt dangerous ſeaſon of the year, and arrived at the coaſt of Caramania, with an intention of traverſing Aſia Minor. As the Engliſh and Algerines infeſted the Levant ſeas, we had with us effects too valuable to be expoſed to danger. This voyage was leſs fatal to Bruguiere, and will be highly valuable to botaniſts; for, beſides various kinds of ſeeds, they will ſee the fruit of the wild plum, which we found very common in Iconium, as far as the environs of Mount Olympus. This tree does not differ from the cultivated. Its fruit, a little larger than the Damaſcus plum, is yellowiſh, with a greater or leſs tinge of red. It is tartiſh, ſomewhat ſour when not fully ripe, but in other reſpects perfectly ſimilar to our ſmall white plums.

During the forced ſtay, which we were in ſome meaſure obliged to make at Conſtantinople, to arrange our ſcattered collection, and to wait for a ſafe conveyance, Bruguiere recovered, if not his former looks, at leaſt a pretty good ſtate of health. Our voyage to Athens, and afterwards to Corfu acroſs the iſthmus of Corinth, and as far as Ancona, was very agreeable, becauſe we had for fellow-travellers men of intelligence.

I accompanied the collection as far as Milan, from which I diſpatched it to Paris. It will no doubt be ſuperfluous to enumerate to you the very valuable objects which I have procured, conſiſting of ſeeds, fruits, drugs, plants, quadrupeds, reptiles, inſects, &c.

XV. *On a New Muſical Inſtrument invented by Dr.* CHLADNI; *with ſome Experiments on the Vibrations of Sonorous Bodies.*

IN the month of February laſt, Dr. Chladni of Wittenberg, well known by his various publications on philoſophical ſubjects, and particularly his experiments on ſound, performed before a numerous aſſembly at Vienna, on a new inſtrument he has invented, and to which he gives the name of *Euphon.* This inſtrument has nothing in common with the *Harmonica* but the glaſs. It conſiſts of forty-one immoveable parallel cylinders of glaſs, of equal length and thickneſs. Its conſtruction, tone, and the method of playing it, are totally different from thoſe of the Harmonica. At the ſame time Dr. Chladni made ſeveral curious experiments on the vibrations of ſonorous bodies, by which he ſhewed that ſand or ſaw-duſt, ſtrewed over theſe bodies while vibrating, forms different regular figures, according to circumſtances. As it may afford ſatisfaction to our philoſophical readers to know the manner in which theſe experiments may be partly repeated, we ſhall ſubjoin the following ſhort account: Take a ſquare piece of glaſs, pretty thin, ſuch as that uſed for windows, about four or five inches over, or even more. Smooth it at the edges on a grinding-ſtone; ſtrew a little ſaw-duſt over its ſurface, and lay hold of it gently with the thumb and fore-finger of the left-hand, ſuppoſing that you lay hold of it by the middle. With the right-hand rub a violin bow ſoftly againſt one of the edges of the glaſs, drawing it either up or down, in a direction almoſt perpendicular to its ſurface, and you will then ſee a tremulous movement and the whole duſt leap about. If the bow is exactly in the middle of one of the ſides, the duſt will arrange itſelf almoſt in the direction of the two diagonals, dividing the ſquare into four iſoſceles triangles. If the bow be applied at a quarter only of the

diſtance

diſtance of the one corner from the other, the duſt will arrange itſelf in ſuch a manner as to be found in the two diameters of the ſquare, dividing it into four equal ſquares. At other times, if the bow deviates a little, it forms a figure like a double C, when the two letters are joined back to back. If the ſquare be held by the two extremities of the diameter, oppoſite to that againſt which the bow is applied, the duſt will form a kind of oval, one of the axes of which will be the ſame diameter. If the glaſs be of a circular figure and be held by the middle, the duſt will arrange itſelf in ſuch a manner as to form the ſix radii of a regular hexagon. In our next number we ſhall preſent our readers with ſome farther particulars on this intereſting ſubject.

XVI. *Account of an extraordinary large Oriental Pearly Excreſcence, which was offered for Sale ſome Years ago at Peterſburgh. From* Neue Nordiſche Beytrage, *by Profeſſor* PALLAS, *Vol. II.*

THIS remarkable production, which was brought from Holland to Peterſburg in the year 1779 in order to be ſold, is repreſented fig. 3, Plate VIII. The poſſeſſor of it at that time was Mr. Daniel Gildemeeſter Janz, of Amſterdam, into whoſe hands it came by bequeſt from one Mr. Sander, an agent of Deux-Ponts, and had been purchaſed in India for 50,000 florins (4,500l. ſterling). It was offered for ſale, under the name of the *Sleeping Lion*, at double the above ſum. It weighed 578 carats, and was quite perfect in its ſhape, as ſeen in the figure; ſo that its name ſeems to have been given it not improperly. The other ſide was ſomewhat flatter, and beautifully marked with zig-zag ſtripes *(guilloſchirt)*: the colour and ſplendour were alſo remarkably fine; ſo that this production ſeems entitled to a place in the firſt claſs of pearly excreſcences; and on account of its ſize deſerves to be made known as a curioſity in

natural

natural hiſtory; though inferior in ſome reſpects to many much ſmaller but regular pearls, ſuch as that in the poſſeſſion of the late Empreſs of Ruſſia, which was worn by Charles XII. in his hat, and afterwards given to his ſiſter as a preſent on her marriage, and which is accompanied with two other pearls not much ſmaller; and the celebrated pearl of the King of Spain, called *La Peregrina* *, purchaſed by Philip II. from a traveller, which weighs 25 carats; is valued at 150,000 dollars, and, in all probability, is the firſt in the world. The above pearly excreſcence ſeemed to have been attached by the ſmall end to a hole in the mother ſhell. On the reſt of the ſurface there was not the leaſt appearance of art having been uſed to give it its ſingular form.

XVII. *Deſcription of the Apparatus invented by Mr.* JOHN CUTHBERTSON *for producing Water by the Combuſtion of Hydrogen Gas in Oxygen Gas.*

A B C D (Fig. 1, Plate VIII.) repreſents the inſtrument and the veſſel *a b c d* in which it ſtands. The glaſs bottle A D, which can contain about 1000 cubic inches of water, has a braſs cap which ſcrews off at A, and is perforated at the bottom, in which the piece *e f* (fig. 2.) is ſcrewed. B and C, two glaſs receivers, with proper mountings, the tops of which paſs through E F, a ſtraight bar of braſs, are made faſt to the bar by female ſcrews put on theſe tops, which are perforated perpendicularly, and have alſo a ſide-hole correſponding with a hole in the braſs bar and with two holes in *e f* communicating with the large bottle. *m* and *n* are two ſtop-cocks in the braſs bar, to ſhut or open the communication between the receivers and the bottle. F R and E N are two flat pieces of braſs made faſt to the veſſel containing the receivers, and which is

* It was a production of the pearl-fiſhery, near the ſmall iſland Margarita, on the coaſt of Comana.

nearly

nearly filled with water, by means of ſcrews at *a* and *b*. O, a metallic wire, is made faſt to the braſs cap A: the lower part of this wire is made of platina, and is brought as near as poſſible to the piece *e f*, but not to touch it. When the inſtrument is to be uſed, the ſtop-cocks being kept ſhut, the large veſſel muſt be detached from the receivers, by unſcrewing the female ſcrews Q Q, filled with oxygen gas by any of the common methods, and again put in its place. The receiver B, which has a hole in its ſide at *o*, muſt then be filled with oxygen gas, and C with hydrogen; and, while electric ſparks are made to paſs from the wire O to the aperture *e*, the cock *n* muſt be opened, by degrees, till the gas takes fire. Stop the electric ſparks and regulate the flame by turning the cock one way or the other. The cock *m*, which ſupplies the oxygen gas, muſt be kept quite open, and the receivers be kept ſupplied, C to its lip and B to the hole *o*, by known meaſures of the gaſes, from time to time, while the proceſs is continued. The paſſage that leads from the hydrogen gas to the large veſſel is made ſmaller than that from the oxygen, that the gas may enter in a very ſmall ſtream. The hole in the ſide of the receiver B is for the purpoſe of preventing more oxygen gas being introduced than will fill it to that point, that the column of water may always be heavier upon the hydrogen gas, which has to force its way through a ſmaller aperture than the oxygen gas. The ſame end would be gained by making B only about half the depth of C. Both theſe receivers are open below, to receive the gas introduced under them.

NEW

NEW PUBLICATIONS.

Kong. Vetenſkaps Academiens Nya Handlingar, &c. New Tranſactions of the Royal Academy of Sciences at Stockholm, for the Year 1797. Vol. XVII. Part Second, with two Plates. Part Third, with three Plates.

THE ſecond part of this work contains the following papers: I. Melanderhjelm's continuation of the Hiſtory of Phyſical Aſtronomy. The author here mentions the attempts of the Academy of Paris, in 1730, to revive the ſyſtem of Deſcartes in oppoſition to Newton's ſyſtem of gravitation, and particularly what John Bernouilli ſays in his New Thoughts on the Syſtem of Deſcartes, and the manner of deducing from it the orbits and aphelia of the planets, together with the objections which he makes to Newton. D'Alembert ſhewed, in his Treatiſe on the Equilibrium and Movement of Fluids, the faults committed by Bernouilli, and proved that, according to Bernouilli's ſyſtem, all the parts of ſuch a vortex muſt complete the revolution of their orbits at the ſame time; and that the planets, if they moved round the ſun in ſuch vortices, muſt deſcribe their orbits in the ſame period; which, however, is contrary to nature. As Bernouilli himſelf found that the velocity of ſuch vortices did not agree with Kepler's rules, founded on obſervation, he endeavoured to explain this difference, but fell into the ſame errors as before. His new hypotheſis alſo, for explaining the motion of the aphelia, was not agreeable to nature; and Bouguer as well as D'Alembert ſhewed that the elliptic orbits of the planets could not agree with Deſcartes's ſyſtem of vortices, and that this ſyſtem could not ſerve to explain their motion.—II. Experiments with *Pietra Fongaia (lapis fungifer)*, and Obſervations on its Nature, by F. A. Gadd.—This ſpecies of earth, found near Rome, Naples and Florence, was firſt diſcovered by Ferber. Near Naples it is found in the chalk-hills, like a white ſtalactites intermixed

with a great many fine roots of ſhrubs; and near Florence there is a ſpecies of it conſiſting of hardened turf, which is dug up near volcanoes. The author made experiments with a piece procured from Italy, and found that 100 parts contain from 45 to 46 ſiliceous earth, 23 argillaceous earth, 7 calcareous earth, and 20 calx of iron, with ſome white magneſia and vegetable alkali. It is well known, that when this friable ſpecies of ſtone is preſerved in cellars and moiſtened with water, it produces abundance of eatable muſhrooms, which, in Italy, are highly eſteemed and brought to the firſt tables. The author conſiders it more beneficial to cultivate morels inſtead of theſe in Sweden, and gives an account of the method.—III. On the Art of hardening Copper, by P. J. Hjelm. Copper, without doubt, was earlier known and employed than iron; and ſome ancient weapons of great hardneſs have given riſe to reſearches in regard to the art of hardening it. The attempts, however, to harden it in the ſame manner as ſteel, all miſcarried, and the art was conſidered as loſt. It was then tried whether this object could not be accompliſhed by mixing it with other metals. Monnet believed that arſenic had been uſed by the ancients for this purpoſe; Geoffroy, iron; Mongez and Dize, tin. The laſt opinion is ſtrengthened by the experiments of M. Hjelm, who having accurately examined the blade of a dagger diſcovered in the earth, found that it conſiſted of $83\frac{7}{8}$ copper and $16\frac{1}{2}$ tin. Of a like mixture of both metals he made blades for knives, which in hardneſs approached very near to the former.—IV. New and little-known Species of Lichens, Fifth Continuation, by E. Acharius. The author here treats of the *Phyſcia foliis membranaceis, tubuloſis, vel longitudinaliter lacunoſe canaliculatis*, to which belong, 1. *Lichen Iſlandicus*; *membranaceus, adſcendens, multifido-laciniatus, ciliatus, canaliculatus, viridi-caſtaneus, ſcutellis ſeſſilibus planis concoloribus*. This ſpecies, beſt known of all on account of its medicinal and nouriſhing qualities, grows abundantly in Sweden. In foreſts covered with moſs it grows to the greateſt ſize on the

cytiſus

cytiſus laburnum, and on ſtones it acquires fine leaves. On the edges it is furniſhed with ſmall ſtiff briſtles, and on the ſides has ſmall excreſcences like the head of a pin beſprinkled with white meal. 2. *Lichen cucullatus*; *membranaceus erectus, glaber, inciſus, tubuloſo-canaliculatus, albus, baſi purpuraſcens, ſcutellis poſticis cucullatis fuſcis.* It is often confounded with the *Lichen nivalis*. 3. *Lichen nivalis*; *membranaceus, adſcendens, laciniatus, criſpus, lacunoſus, albus baſi flaveſcens.* Moſt botaniſts have conſidered this as the *Lichen cucullatus*. It grows farther towards the north. The organs of fructification are little known. 4. *Lichen tenellus*; *membranaceus, decumbens laciniatus, laciniis apice ciliatis obtuſis, tubuloſo-fornicatis, adſcendentibus, cineraſcens*; *ſcutellis ſeſſilibus, lateralibus, nigrocæſiis.* It has long been conſidered as a variety of the *L. ciliaris*; from which, however, it is very different. 5. *Lichen ciliaris*; *membranaceus, adſcendens, linearis, laciniatus, ciliatus viridi-glaucus, ſubtus albus, canaliculatus, ſcutellis terminalibus nigrocæſiis.* It grows particularly on the aſh. 6. *Lichen furfuraceus*; *membranaceus, decumbens laciniatus, pulverulentus cinereus, ſubtus canaliculatus violaceo-niger, ſcutellis ſparſis ampullaceis rubris.* It grows on the fir, birch, and ſtones; contains abundance of inflammable particles, and reſin.—V. Deſcription of the *Picus Javanenſis*, a new Bird from Java, by S. J. Liungh; with a Figure. It is a kind of woodpecker with three toes, the characters of which are as follow: *Picus Javanenſis, vertice criſtato nigro albo maculato, collo ſubtus albo, lineis ſex nigris, dorſo ſuperius flavo, inferius cinnabarito, pedibus tri-digitatis.*—VI. Deſcription of an Apparatus for planing Ship Pins, invented and employed at Carlſcrona, in 1784, by C. F. Bouke, with a figure. This machine is ſo conſtructed that it will make theſe pins either round or ſquare. Two workmen in a day can make with it four or five times as many as by the uſual apparatus with a hand-plane. At Carlſcrona, where 35000 ſuch nails of oak or fir are uſed every year, this machine cauſes a ſaving of 666$\frac{2}{3}$ rix-

dollars in point of labour.—VII. Observations on the Strength of the Walls of Fortifications, by A. Sjöberg. The author prefers the theory of Stahlfverd, in regard to fortification, to those of Lorgna, Delanges, and Woltmann: a more accurate account of this theory has been given in Bohm's Magazine for Engineers; but Stahlfverd is too short, and in some parts he has not sufficiently explained his calculations. He has also employed fluxions sometimes with long converging series, though it might have been possible by other methods to approach nearer to the object. M. Sjöberg, therefore, has begun here to improve and explain this theory.—VIII. Farther Examination of the Black Stone of Ytterby, and the peculiar Earth found in it, by A. G. Ekeberg. The author continued the experiments made by Gadolin on this subject, and found in 100 parts 47½ of a peculiar earth, different from all the other kinds of earth hitherto known, and to which he gives the name of Ytter earth, *Yttrica*. The stone itself he calls Ytter stone.

In the third part we find the following papers: I. Some new Moths of the Genus of the *Tortrices*, by C. P. Thunberg, such as, 1. *Tortrix Fahlbergiana: alis basi cinereis apice purpureis; strigis albidis*; from the island of St. Bartholomew in the West Indies. 2. *T. Stickmanniana, alis externe fuscis interne albis, linea media ferruginea abbreviata.* 3. *T. Liunghiana: alis albis punctis fuscis irroratis; fasciis tribus fuscis.* 4. *T. Achariana: alis plumbeis, fasciis duabus nigris, inferioribus margine albis.* 5. *T. Blomiana: alis plumbeis superioribus, arcu fasciaque abbreviata obliqua atra.* 6. *T. Naezeniana, alis purpureo-aureis; maculis octo flavis, antennis annulatis.* All these are found in Sweden, and have been named after the Swedish naturalists by whom they were discovered. II. *Westringia*, a new Genus of Plants described by J. E. Smith, M.D. President of the Linnæan Society of London. It was first discovered in New Holland by Dr. Solander, who called it *Cunila fruticosa*, though it is totally different, and has rather a similarity to rosemary, from

which,

which, however, it is different alſo. Its peculiar character is: *Calyx ſemiquinquefidus, pentagonus; corolla reſupinata, limbo quadrifido, lobo longiore erecto, bipartito: Stamina diſtantia, duo breviora (inferiora) abortiva.* The author aſſigns it rather to the *didynamia-angioſpermia*, immediately after the *teucrium*, than to the *diandria*. III. Experiments on the method of preparing, from moſt of the lichens, dyes for giving beautiful high colours to ſilk and wool. By D. Weſtring, M.D. Sixth continuation. Of theſe lichens, fit for the purpoſes of dyeing, there are eleven or twelve kinds, a great many of which grow in Sweden, ſo that large quantities of them might be collected every year. They afford excellent nouriſhment to goats. In ſome places they are employed as ſtuffing for chairs and ſophas, in the room of horſe hair, &c. ſuch, for example, as the *Lich. chalybeiformis, barbatus* and *plicatus*. Some kinds, ſuch as the *L. barbatus* and *plicatus*, are uſed by the common people as medicine in the hooping-cough, hemorrhages, jaundice, &c. for which the *L. hirtus* is alſo uſed. The author was at great pains to examine theſe different ſpecies, and made upwards of two hundred experiments with them in dyeing, but particularly with *L. jubatus; filamentoſus pendulus axillis compreſſis: L. chalybeiformis; filamentoſus ſubramoſus decumbens, implicato-flexuoſus; and L. lanatus, filamentoſus, ramoſiſſimus, decumbens, implicatus, opacus.* The firſt gives a kind of orange-red dye (*color aurantiacus*), and particularly to yarn; the ſecond does not yield ſo good a dye; and the third is of no uſe for dyeing. IV. Deſcription of new and little known ſpecies of lichens, by E. Acharius. Sixth continuation. The author here treats of the *phyſcia foliis ſubcartilagineis, planis aut convexiuſculis*, of which figures are given, and among which, beſides thoſe mentioned in the preceding part, the author enumerates, 7. *Lichen prunaſtri: ſubcoriaceus mollis erectiuſculus lineari laciniatus planus rugoſo-lacunoſus, ſubtus albiſſimus; ſcutellis lateralibus concavis fuſcis.* 8. *Lich. fraxineus; cartilagineo-membranaceus erectus planus, lanceo-*

latus rugoſo-lacunoſus cinereus ; ſcutellis ſparſis lateralibus, marginalibuſque pallidis. 9. *Lich. faſtigiatus : ſubcartilagineus erectus cæſpitoſus ramoſus ; ramis faſtigiatis tertiuſculis lacunoſis ſurſum incraſſatis ſubinermibus ; ſcutellis ſeſſilibus terminalibus.* 10. *Lich. calicaris : ſubcartilagineus erectus linearis ramoſo-pinnatifidus, compreſſus lacunuloſus pallidus ; ſcutellis pedunculatis convexis ſparſis.* V. Farther examination of Stählſwerd's theory reſpecting the ſtrength of the walls of fortifications, by A. Sjöberg, with figures. VI. Geometrical eſſay on the motion of bodies drawn from or towards a given point in the inverſe double ratio of the diſtance from that point, by J. Svanberg. Neither of theſe papers, which are to be continued, will admit of abridgement.

Afhandling om. Telegrapher och förſök til en ny inrättning däras, &c. A Treatiſe on Telegraphs, and a new Conſtruction of one, by A. N. Edelcrantz, Counſellor of the Chancery, one of the Eighteen of the Swediſh Academy, &c. Stockholm, 1796. 95. p. 8. with five copper-plates and three tables.

When people wiſh to tranſmit intelligence to others, at a diſtance, in a quicker manner than by letters or meſſengers, it can be done only by ſignals, but two different ways: either by ſingle ſignals, which, according to previous agreement, convey whole ideas ; or by ſeveral ſucceſſive ſignals, which, by repreſenting letters and words, anſwer the ſame purpoſe. The firſt kind of ſignals were employed in the earlieſt periods, and ſome of them were ſuited to the ear as well as to the eye. For the eye the ancients employed fires and ſmoke, torches, flags, and, in modern times, ſky-rockets have been uſed. For the ear they employed drums and trumpets; and, ſince the invention of gun-powder, the firing of cannon has been applied to the like uſe. But even all theſe are incapable of expreſſing what could be communicated by ſpeech and writing; and to accompliſh that object, by ſigns capable

of

of expreſſing all the poſſible variations and combinations of the letters of the alphabet, forms, in a proper ſenſe, the true telegraphic art. Even the ſignals at ſea, as they extend only to particular circumſtances, are, when compared to ſignals by letters, only a kind of hieroglyphics.

The proper telegraphic art, however, was not unknown to the ancients. The Greeks and the Romans, for ſignals, made uſe of pots filled with lighted twigs and ſtraw, over which they poured oil; and theſe being placed in certain rows, expreſſed certain letters, according to the order in which each row was lighted. Polybius mentions a method in which the letters of the alphabet were divided into five rows; and by means of a certain number of torches, raiſed on the right and left, the number of the row, and the letter in the row, could be expreſſed. The *ſteganographia trithemiana* of a benedictine monk, in the 15th century, ſeems to have been ſomething of the ſame kind; but the firſt experiment, after the manner of the Greeks, is deſcribed by Kircher in his *Ars magna lucis et umbræ*, under the title of *Cryptogamia catoptrica*. It was, however, imperfect, and could be employed only at a certain diſtance. Schott in his *Technica curioſa* propoſes, from an anonymous author, to erect poſts upon an eminence, ſo as to be diſtinguiſhed through a teleſcope, and on which proper ſignals could be elevated as might be neceſſary. Keſsler, in his Concealed Arts, adviſes characters to be cut out on the bottom of a caſk, ſo as to appear luminous; and Hook propoſes certain figures, the form of which could be eaſily changed, to be ſuſpended on a wooden frame. Gautkey recommends tubes ſome thouſand fathoms in length; Guyat, tables with the letters cut out in them; and Paulian, in his *Dictionnaire de Phyſique*, a tranſparent figure conſiſting of one perpendicular and three horizontal ſtripes forming ten compartments, each of which, by removing boards from behind, can be rendered viſible or inviſible. The inventions of Linguet, Bergſtraſſer, Chappe, Burja, Bockman, and Achard, were all much on the ſame

plan. After this hiſtorical introduction, the author deſcribes the properties of a good telegraph. Theſe are: 1ſt, perfection in regard to the combination of the characters and the property of the ſigns; 2d, perfection in regard to eaſineſs of movement and uſe. Theſe properties, the author ſays, are united in that of Chappe. His telegraph conſiſts of three arms, the inclination of which towards each other, towards the perpendicular poſt, and towards the horizon, give certain poſitions that repreſent the old Runic letters, or the oldeſt alphabets, and by their combinations 256 poſitions or particular ſigns are poſſible. In September 1794, the author began to make experiments in Sweden, before he was acquainted with a complete deſcription of the French telegraph; and he contrived various forms, ſome like the French and ſome entirely different, till he at length fell upon that which he here deſcribes, the combination and uſe of which are attended with great advantages. A deſcription of it without the figures would require too much time. It is properly a ſort of lattice with ten apertures in three vertical rows, which can be opened and ſhut by means of boards*, and which are capable of forming 1024 combinations or diſtinct ſignals. Theſe, in order to be eaſily diſtinguiſhed and repeated, are expreſſed by figures arranged in tables ſubjoined to the work; ſo that, with theſe 1024 ſigns, it is poſſible to repreſent 2097152 variations of the figures. For ſignals in the night-time a lamp is applied behind each hole, and covered or uncovered as may be neceſſary.

* This ſeems to be on the ſame principles with thoſe adopted in England. EDIT.

INTEL-

INTELLIGENCE

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ROYAL SOCIETY OF LONDON.

THE meetings which took place on the 6th and 13th inſtant were occupied by the reading of a paper on Viſion, by Dr. Wilſon, of which we find it impoſſible to give any abſtract. At the latter meeting the Annual Regiſter of Meteorological Obſervations was read to the Society.

At the meeting on the 20th, a paper was read containing experiments on vegetation in pure water, by Dr. Crell. He planted the ſeeds of ſunflowers in ſome ferruginous ſand which he had ſeveral years employed for ſand heats, and which, he therefore ſays, could contain no carbon. The *pure* water was that with which his place of reſidence is ſupplied by pipes. The pot in which the ſeeds were planted was put into his library, which, being but ſeldom opened, *he* concludes, could therefore contain no carbonic acid gas (fixed air). Four of the ſeeds grew and flowered: the ſeeds of one of which were again reared in the ſame manner in the following year. The queſtion put by Dr. Crell is: Whence do vegetables derive their nouriſhment? and particularly, whence comes the carbonaceous matter which conſtitutes ſo large a portion of their bulk? After ſome reaſoning on the ſubject, the author concludes, that carbon is either depoſited in plants by the light which falls upon them, and is therefore a conſtituent part of light, or that it is generated in them by its action.

The reader will naturally aſk, How water coming from a common reſervoir can be conſidered as pure? How ſand

which has been employed for the purposes of a sand heat for years, and consequently exposed to receive dust and small fragments of coals, can be free from carbon? And what sort of a room it must have been which could contain no carbonic acid gas in the atmospheric air with which it was filled, places but seldom opened being generally found to contain more of that gas than those which are better ventilated?

FRANCE.

The Society of Natural History at Bourdeaux has formed itself into a Society of the Sciences, Belles-Lettres and Arts; and, like the National Institute, is divided into three classes: 1st, Mathematical and physical sciences; 2d, Moral and political sciences; 3d, Literature and the fine arts.

The Society of Natural History at Paris, has given itself a new organization. In future it will assemble only once a month; but it has chosen an administration who will meet once every fortnight. This administration consists of six commissioners, Jussieu, Lamarck, Hauy, Fourcroy, Desfontaine and Lacepede, who have chosen six assistants: Ventinat, Brogniard, Leliere, Vauquelin, Celse and Millin. Cit. Sylvestre has been appointed treasurer, and Cit. Cuvier secretary. The rest of the Society is divided into two sections: one of members, each of whom engages to read a memoir annually; and the other of free associates, who are not obliged to perform any labour. The Society have published a volume of Memoirs in folio, under the title of *Acte de la société d'histoire naturelle*, and has erected a statue of Linnæus in the national garden of plants.

MISCELLANEOUS.

PRESENT STATE OF CHEMISTRY IN GERMANY.

As we have frequent occasion, in the course of this Magazine, to mention or make extracts from the works of different chemists on the Continent, and as they have not yet all entirely abandoned the old system, our chemical readers, we hope,

hope, will not be diſpleaſed with the following ſhort account of the preſent ſtate of chemiſtry in Germany, being an extract of a letter from Dr. Girtanner to Van Mons, dated Gottingen, Sept. 5, 1796, publiſhed this year, in one of the German Journals, with ſome obſervations by Van Mons.

"The ſyſtem of chemiſtry in Germany has undergone a revolution. The exiſtence of phlogiſton is no longer defended, but by Gren (a man of diſtinguiſhed talents, though obſtinate), Weſtrumb, Gmelin and Crell. The three laſt have declared eternal war againſt the anti-phlogiſtic doctrine. Their enthuſiaſm even proceeds ſo far that, if we ſtill lived in the age of martyrdom, they would not heſitate to ſuffer themſelves to be burnt, in order to prove the exiſtence of phlogiſton in their bodies. Trommſdorf, who has embraced the new doctrine, ſtill adheres, however, in ſome ſmall points to the old. Göttling defends his new hypotheſis. Among our young chemiſts, Scherer of Jena promiſes great things. He is a zealous follower of the French chemiſtry, a good experimenter, and poſſeſſed of great knowledge: there is nothing but the *res anguſta domi* which prevents him from labouring and making new diſcoveries. Profeſſor Mayer at Erlangen is a man of great talents, who to natural philoſophy and chemiſtry unites a profound knowledge of the mathematics like Monge at Paris. Hermbſtädt does every thing he can for the advancement of the new doctrine. The above is a faint ſketch of the preſent ſtate of chemiſtry in Germany.

"As my occupations have not permitted me to continue my experiments on phoſphorus and alkalies, I am unwilling to communicate to you thoſe I have made, until I repeat them: I will, however, venture to aſſure you, that phoſphorus is a compound of azot and hydrogen. All phoſphorus contains more or leſs carbon; but, in my opinion, that principle does not enter into its compoſition.

"I have proved that phoſphorus may ſhine in azotic gas and carbonic acid gas by means of the water which theſe

gaſes

gaſes contains, and which the phoſphorus decompoſes; as is proved by the phoſphorated hydrogen gas obtained in theſe experiments. I explain by this the experiments of Göttling. I wait with impatience for the memoir you have announced, and in which you have proved that I was deceived in regard to the muriatic radical. I ſhall repeat the experiments; and whatever be the reſult, ſhall render homage to truth."

The following obſervations on the above letter have been publiſhed by Van Mons: "This letter might induce people to believe that the four chemiſts, of whom Girtanner ſpeaks, ſtill profeſs the principles of the theory of Stahl. On this ſubject I ought to undeceive thoſe who have been prevented by the war from being fully acquainted with the progreſs of the new chemiſtry in Germany. That country has ceaſed to have among its chemical writers any partizan of the unqualified ſyſtem of phlogiſton, ſince I convinced them of the preſence of oxygen in the oxyd of red mercury. They have all adopted the new doctrine without reſtriction, or with reſtrictions of very little conſequence. Crell, Weſtrumb, Wiegleb, Trommſdorf, Gmelin, Richter, Leonardi, &c. in endeavouring to unite the new theory with the exiſtence of phlogiſton in combuſtible bodies, all admit it in general and all its conſequences. They wiſh only to ſave themſelves from the appearance of a complete defeat. Thoſe who ſtill adhere to the preſervation of that agent, conſider it as the baſis of light, or as dormant light. This baſis, abundantly contained in inflammable ſubſtances, when it meets and combines itſelf with caloric, conſtitutes luminous fire; which explains why combuſtible ſubſtances require a certain degree of heat before they are inflamed. This ſyſtem, and the applications of it which follow, are the only points in which they differ from us: in all the other explanations they agree perfectly with our principles. We ſhall ſee how little influence theſe theories modified will have over the future progreſs of ſound chemiſtry in Germany.

"Gren,

" Gren, as profound a philoſopher as a chemiſt, a great mathematician and geometrician, no longer attaches any importance to the admiſſion of a particular inflammable matter. His Foundations of the New Chemiſtry, the firſt volume of which he has juſt publiſhed, are entirely written according to the principles of the French ſyſtem. In his Manual of Chemiſtry, printed two years ago, he gave the theory of oxygen along with that of phlogiſton. He did almoſt the ſame thing, the preceding year, in the ſecond edition of The Grounds of Natural Philoſophy. A third edition of that work is now printing, in which he will give an account of the phenomena of that ſcience according to the pure ſyſtem of Lavoiſier. His Journal of Natural Philoſophy, of which eleven volumes have appeared, has always admitted indiſcriminately articles for and againſt both theories. That Gren ſhould ſo long doubt, can aſtoniſh thoſe only who are ignorant, that to think for oneſelf gives birth to ſcepticiſm and diverſity of opinions.

" Gmelin is excluſively employed, as we may ſay, with hiſtorical and technical chemiſtry. In the ſecond edition of his Manual of Chemiſtry, as applied to the Arts, which he has juſt finiſhed, he gives the little theory required in ſuch a work according to the old principles; but in addreſſing himſelf to beginners, ought he not to ſpeak in language that is known and ſuited to their comprehenſion? His Introduction to General Chemiſtry gives an account of the ſtate and progreſs of the ſcience in both theories.

" Weſtrumb gives alſo many articles of technical chemiſtry, which he treats of with a knowledge and diſcernment which denote a chemiſt well verſed in the practical part of his art. His writings on pharmacy evidently ſhew that they are the work of a man who has ſeen much and reflected well on an art where a great deal ſtill remains to be done. In both theſe kinds of labour he prefers facts to reaſoning.

" Crell is ſtill the editor of the Chemical Annals; a valuable collection, which has tended much to promote the culti-

vation

vation of chemiſtry in Germany; and in which he gives, with fidelity, not only the opinions, but even the language of the authors. He ſtill finds it difficult to adopt our principles; but at the age of that reſpectable literary veteran it is not eaſy to abandon old ideas. Beſides, Crell has among his ſubſcribers and correſpondents ſome diſciples of Stahl, whoſe ſupport he muſt endeavour to retain."

EARTHQUAKE IN SOUTH AMERICA.

A letter, dated Quito, February 20, 1797, contains the following particulars reſpecting this dreadful cataſtrophe, ſome account of which has already appeared in different gazettes:

"Hiſtory affords no example of a devaſtation like that which lately took place in the provinces of Taninga, Ambato, Riobamba, Alaoſi, part of Chimbo and Quito. Not a houſe has been left ſtanding: every thing is levelled with the ground. We are aſſured, that the volcano of Maas made a moſt dreadful eruption, and ſplit in two parts, which ſeparated from each other. An earthquake, which took place at the ſame time, ſhook the mountains with ſuch violence that they were daſhed againſt each other, and threw up ſometimes ſtones with clouds of duſt, and ſometimes ſtreams of burning lava, or water. The mountain Ygualata in falling threw out a ſtream of lava which rolled along with fiery waves, and which in its courſe totally deſtroyed Capalpi, San Andreas, Ouaono, Emlyies, Guanando, and ſeveral other places. The mountain Moya almoſt diſſolved into water, and ſwept away Pelile and the celebrated plantation of St. Ildephonſo, where a thouſand perſons periſhed. The mountain Euero fell on the village of the ſame name, without leaving a ſingle witneſs of this melancholy cataſtrophe. The mountain Yatagui fell upon Maſoro, and opened ſuch a dreadful gulph, that every thing, houſes, churches, and inhabitants, all but two perſons, were ſwallowed up by it. This place was converted into a bituminous lake, which

emitted

emitted a ſulphureous vapour. The ruins are every where ſo great, that all the gold and ſilver of America will hardly be ſufficient to repair the devaſtation. The number of thoſe who have loſt their lives cannot be aſcertained; but it muſt be very conſiderable. The provinces of Riobamba, &c. have ſuffered moſt: in the laſt many died of hunger, and even of thirſt, as all the water was corrupted. In other diſtricts the ſhocks ſtill continue; and the lakes throw up flames. At Sambagna and Timba new rivers have burſt forth at different places."

SINGULAR MAGNETIC PHENOMENON.

In the *Reichſanzeiger*, a German periodical work, No. CCXXII. for 1797, it is ſaid, that a certain perſon having an artificial magnet ſuſpended from the wall of his ſtudy with a piece of iron adhering to it, remarked, for ſeveral years, that the flies in the room, though they frequently placed themſelves on other iron articles, never ſettled on the artificial magnet; and even that if any of theſe inſects approached it, they in a moment again removed from it to ſome diſtance. "It is worth the trouble," ſays Profeſſor Voigt, who repeats the ſame circumſtance in his Journal, "to make farther obſervations on this phenomenon; and, were it confirmed, to magnetiſe iron might be employed to preſerve it from being dirtied by flies. Perhaps it might be employed alſo for other purpoſes."

VENTILATION.

Dr. Van Marum has diſcovered a very ſimple method, proved by repeated experiments, of preſerving the air pure in large halls, theatres, hoſpitals, &c. The apparatus for this purpoſe is nothing but a common lamp, made according to Argand's conſtruction, ſuſpended from the roof of the hall and kept burning under a funnel, the tube of which riſes above the roof without, and is furniſhed with a ventilator. For his firſt experiment he filled his large laboratory with

the ſmoke of oak ſhavings. In a few minutes after he lighted his lamp the whole ſmoke diſappeared, and the air was perfectly purified.

MEDICINE.

A letter from Dr. B. Lynde Oliver, of the ſtate of Maſſachuſetts in North America, to a medical gentleman in this country, who has favoured us with a peruſal of it, contains the following information, which may prove acceptable to practitioners:

"I have this ſeaſon," ſays he, "had great ſucceſs in exhibiting Dr. Wright's medicine, viz. Vinegar ſaturated with common marine ſalt, and diluted with thrice its quantity of hot water, in dyſentery. Dr. Perkins had the merit of introducing this compoſition in that particular diſeaſe. From the trials I have made of the efficacy of this medicine, I am almoſt inclined to believe that it will ſuperſede the uſe of all other remedies. I have ſeen patients apparently ſnatched by it from the jaws of death. I generally join a little laudanum with it, but firſt abate the violence of the diſeaſe by a few doſes of the mixture. Oftentimes the firſt doſe has given a favourable turn to the diſeaſe."

The following is a copy of Dr. Perkins's notice publiſhed on this ſubject:

A uſeful Treatment for the Dyſentery, and Scarlatina Anginoſa or Canker Raſh.

IN the courſe of an extenſive practice for about five years paſt, I have met with the moſt pleaſing ſucceſs in treating the dyſentery and ſcarlatina anginoſa; and as theſe complaints frequently extend their baneful ravages, I take pleaſure in communicating to the public a ſimple remedy, which, when judiciouſly uſed, has ſeldom failed of removing thoſe diſeaſes.

Saturate any quantity of the beſt vinegar with common marine ſalt; to one large table-ſpoonful of this ſolution, add four times the quantity of boiling water; let the patient take of this preparation, as hot as it can be ſwallowed, one ſpoonful

ful once in half a minute until the whole is drank: this for an adult. The quantity may be varied according to the age, ſize, and conſtitution of the patient. If neceſſary, repeat the doſe once in ſix or eight hours. Conſiderable evacuations I conceive to be not only unneceſſary, but injurious, as they ſerve to debilitate and prolong the diſeaſe.

A tea of plantain, or ſome other cooling, ſimple drink, may be uſeful; and if a thirſt for cyder be diſcovered, it may be gratified.

Carefully avoid keeping this preparation in veſſels partaking of the qualities of lead *, as the poiſon produced by that means may prove dangerous.

The ſucceſs of the remedy depends much on preparing and giving the doſe, as above directed.—The ſimplicity of this treatment renders it the more valuable, as all perſons have it in their power to avail themſelves of its uſe.—I have found it uſeful in agues, diarrhœas, and the yellow fever.

DISTANCE OF COMETS FROM THE EARTH.

Mr. Proſerpin, who in the years 1775 and 1785 publiſhed in the Tranſactions of the Swediſh Academy of Sciences, papers on the leaſt diſtance of comets from the earth, has publiſhed, in the XVIIth Vol. Part I. of the ſame work, a continuation of that ſubject, in which he gives a calculation of all thoſe that have appeared ſince 1785. He enumerates the different comets obſerved by Saron, Mechain, Von Zach, Maſkelyn and Bode, and marks their leaſt diſtance from the earth in an annexed table. It appears from this paper, that none of theſe eleven comets can approach very near to the earth. That, even, which paſſes at the leaſt diſtance, cannot approach nearer to it than about thirteen times the diſtance of the moon from the earth; and the two next, when neareſt to us, are twenty or twenty-ſix times as far from the earth. Beſides theſe, three only can

* This caution ought to be extended to ſome other metals alſo. EDIT.

approach nearer the earth than Venus; and two cannot approach ſo near as Saturn when at his greateſt diſtance.

REMARKABLE INSTANCE OF LONGEVITY.

In a ſmall village not far from Bergen in Norway, died in October 1797, Joſeph Surrington, in the 160th year of his age. He retained the perfect uſe of his ſenſes till the laſt hour of his life. The day before he died he aſſembled his family and divided his property amongſt them. He had been ſeveral times married; and left behind him a young widow and ſeveral children. His eldeſt ſon is 103, and his youngeſt nine years of age. *Bayreuth Gazette, No.* 216. 1797.

Our readers are requeſted to make the following corrections in our laſt Number. Page 188, 5th line from the bottom, for *cured a pain*, read *diminiſhed a pain*. Page 189, l. 9, after *merit of being*, inſert *more than*.

THE PHILOSOPHICAL MAGAZINE

JANUARY 1799.

I. *Observations on Fire-Balls and Hard Bodies which have fallen from the Atmosphere. By Dr.* CHLADNI *of Wittemberg. From Professor* VOIGT's Magazin für der Neuesten Zustand der Naturkunde, 1797.

SINCE I wrote my last essay on this subject *, several new circumstances have been made known, which seem still farther to confirm my theory. One of the most interesting phenomena of this kind was the shower of stones near Sienna, on the 16th of June 1794, of which an account has been given in the works of several Italian and other philosophers; and in a paper, by Mr. Zolner, in the Berlin *Monatschrift* for September 1796. About seven o'clock in the evening of that day there appeared, at the above place, a longish, round, dark cloud, totally insulated, which by its singular appearance excited fear as well as attention, and was observed, at the same time, at various places lying at a considerable distance from each other. All of a sudden there fell, amidst the most dreadful explosions, accompanied with lightning and a kind of vapour or smoke which burst from

* See Philosophical Magazine, vol. ii. p. 225.

the clouds, a multitude of red hot ſtones of the vitrified kind, moſt of them quite ſmall, but ſome of them a pound in weight, many of which ſunk to the depth of more than an ell in the earth. One of them pierced the brim of a boy's hat and ſinged the felt: others, which fell upon trees, left traces of their ignition on the leaves. Some of conſiderable ſize, which fell in a pond, ſplaſhed the water out to a conſiderable diſtance, and made it even begin to boil. The government found means to drain the pond in order to get at the ſtones, which were purchaſed at a dear rate by ſome Engliſh travellers then in that part of the country; and on this account many attempted to make ſuch ſtones artificially, ſo that people in purchaſing theſe curioſities were frequently in danger of being cheated.

It was at firſt believed that this phenomenon might have had ſome connection with the eruption of Veſuvius, which took place the day before; but the Italian naturaliſts all now agree that this opinion is entirely void of foundation, as the ſtones which fell from the atmoſphere had no ſimilarity to the volcanic ſubſtances thrown up by Veſuvius, and as the place where they fell was above 200 miles diſtant from that mountain. A ſtone of this kind, which was ſubjected to examination, was inwardly of an aſh grey colour; and had a dull earthy fracture mixed with ſhining metallic particles which reſembled pyrites. In other ſpecimens there were found octoedral cryſtals, which were found to be magnetic iron. The colour on the outſide was a greyiſh black, and the wrinkled ſurface betrayed marks of fuſion.

This ſubſtance, in all probability, was of the ſame nature as that which fell, with a dreadful exploſion, from a fire-ball (*bolis*) in Gaſcony on the 24th of July 1790, as mentioned in the *Decade Philoſophique* of February the 29th, 1796, and as many others of the like kind quoted in my treatiſe on the iron maſs found by Profeſſor Pallas, and on ſimilar bodies. Since that period two new circumſtances of this nature have been made known. In the Engliſh journals there is an account

count of a ſtone weighing fifty-ſix pounds, which fell from the atmoſphere with a loud report at Wold Newton in Yorkſhire, on the 13th day of December 1795. As it ſunk only eighteen, or according to others twenty-one inches into the earth, it is to be conjectured that the ground was extremely hard, or that the ſtone did not fall in a ſolid body, but in a ſoft liquid maſs. It was ſtill warm to the touch when found; ſeemed externally of a black colour, and in the inſide exhibited ſhining particles and traces of ſulphureous vapours. In the Gentleman's Magazine, for September 1796, there is an account of ſome ſtones which fell near Petreſwood, in the county of Weſtmeath in Ireland, in the year 1779, accompanied by a violent clap of thunder. Theſe ſtones, which had no likeneſs to any of the known foſſils in that neighbourhood, weighed only $3\frac{1}{2}$ ounces; they were almoſt like free ſtone, of a whitiſh brown colour, and in the inſide were interſperſed with ſilvery white, ſhining ſpecks. They were ſtill warm when taken up, and after they fell the whole neighbourhood was filled with ſulphureous vapour. In a work publiſhed at London in 1796, entitled, Remarks concerning Stones aid to have fallen from the Clouds, by Edward King, with which I am acquainted only by the Engliſh Reviews, an account is given of the above phenomenon, and of ſeveral others of the like kind. Intereſting obſervations by Mr. Böttiger of the accounts given in ancient authors of ſtones ſaid to have fallen from the heavens, may be found in the German *Monatſchrift* for Auguſt 1796, where the author ſhews that Anaxagoras explained this phenomenon in the ſame manner as I have done. The caſe with this conjectural theory of mine is the ſame as with the theory of comets, which by Seneca were conſidered as regular planetary bodies*; but by others afterwards as terreſtrial meteors, till more accurate obſervation ſhewed that Seneca was right.

* Quæſt. Nat. lib. vii.

The only inſtance of a ſoft, frothy maſs being found in the place where a fire-ball fell is that which occurred in Luſatia, and the neighbouring diſtricts, on the 8th of March 1796; and of which an account was given, in particular by Mr. Von Gerſdorf and Mr. Bauer, in the Luſatian *Monatſchrift* for April and May 1796. The fallen maſs appeared to be only a part which had been broke off, or which had, in ſome meaſure, dropped from one of theſe fire-balls. It is much to be lamented that the apparent courſe of this body, as far as could be done by the eye, was not obſerved with ſome accuracy, as has been the caſe with others mentioned in my Treatiſe upon Iron Maſſes, &c. that by calculating their parallax their real path might be diſcovered. Its velocity muſt have been as great as that of many other fire-balls, which have moved at the rate of ſeveral German miles per ſecond, and equal to the velocity of the planets in their courſes. This fire-ball, as was the caſe with that obſerved on the 22d of July 1762, and with others, was firſt viſible at a conſiderable height above the horizon in the form of a ſhining point; moved along in a ſomewhat ſerpentine direction, and increaſed ſuddenly to a conſiderable ſize: the duration of the whole phenomenon was about two ſeconds.

It is much to be wiſhed that ſome one could give a more accurate account of the fire-ball which, as I have been informed, fell a few years ago in the ſea near Genoa, and occaſioned a ſtrong ebullition in the water.

I conſider it as not ſuperfluous to give here a ſhort account of ſome more inſtances of ſtones and maſſes of iron which have fallen from the atmoſphere, as mentioned in my Eſſay, becauſe ſome, on account of my apparent paradoxical method of explaining theſe phenomena, may have been prevented from reading that work; and to take ſome notice of the circumſtances attending them, which, on account of their great conformity, cannot be denied. A very remark-

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able inſtance, which is related by Mr. Stütz, took place at ſix o'clock in the evening on the 26th of May 1751, in the diſtrict of Agram in Sclavonia. This fire-ball, which burſt aſunder into two parts, that had the form of chains of fire twiſted together (and, according to all appearance, were liquid maſſes), ſeemed accompanied with ſmoke, and fell with a dreadful exploſion and ſuch force that it ſhook the earth, into which it ſunk to the depth of three fathoms. One part weighed 71 and the other 16 pounds. The larger, which conſiſts entirely of iron, and exhibits on its ſurface the moſt evident marks of fire, is now to be ſeen in the Imperial collection of natural curioſities at Vienna, together with an atteſtation reſpecting it from the Conſiſtory of the Biſhopric of Agram, who examined ſeveral eye-witneſſes that had ſeen it, and which Mr. Stütz has inſerted in his paper. The ſame naturaliſt quotes alſo two other inſtances. One is of a ſtone, which, according to the account given, fell with a violent exploſion in the diſtrict of Eichſtadt, and was ſo hot that it could not be touched till it had been cooled in ſnow: this ſtone conſiſts of ſiliceous earth and ferruginous particles, and is covered with a cruſt of native iron. The other is a ſhining iron ore deſcribed by Born *, interſperſed in a green matrix having a ſcoriaceous ſurface, and which, as we are aſſured, fell from the heavens during a thunder-ſtorm on the 3d of July 1753.

In the Hiſtory of the Academy of Sciences for 1769, we find very remarkable accounts of three maſſes which fell from the clouds during a thunder-ſtorm. Theſe accounts were tranſmitted to the Academy by their correſpondents from Maine, Artois, and Cotantin, three places lying at a very great diſtance from each other. The ſame circumſtances were remarked to have taken place in regard to theſe three maſſes. They were all hot when firſt found; they were of the ſame compoſition, as they contained ſulphur and iron, and were covered with a hard cruſt of that metal. They

* Index Foſſilium, vol. i.

proceeded, in all probability, from the ſame meteor which in the courſe of its ſomewhat ſerpentine courſe, as is uſual, may have made ſeveral exploſions. Ancient authors, ſuch as Pliny, Plutarch and Livy relate various inſtances of ſtones that fell from the heavens, which are commonly conſidered as fabulous; but which, on account of the number of new obſervations of the like kind, may be conſidered as real phenomena of nature. Avicenna mentions a ſulphureous kind of ſtone which fell from the atmoſphere at Cordova in Spain *. It is related in Spangenberg's *Chronicon Saxonicum*, that two large ſtones fell at Magdeburg, in the year 998, with loud exploſions like thunder. Cardan relates †, that in the year 1510 ſeveral large ſtones fell from the heavens, which were moſtly of a ferruginous colour, exceedingly hard, and, when found, ſtill exhaled a ſulphureous vapour. We are aſſured by Julius Scaliger ‡, that he himſelf had in his poſſeſſion a piece of iron which fell from the atmoſphere in Savoy. Muſchenbroek § and various other writers mention a blackiſh ſtone of 300 pounds weight exhibiting traces of fire, which fell at Enſiſheim in Alſace in 1492 or 1493, and which was ſaid to be preſerved in the church of that place. In the ſummer of the year 1766 a ſtone fell from the clouds at Alboreto, in the duchy of Modena, which is briefly mentioned by Vaſſalli in his Phyſico-meteorological Letters ‖, and on which Troili has written a treatiſe. In the Breſlau collection reſpecting natural hiſtory four inſtances are quoted of ſtones having dropped from the heavens. Five ſtones, as large as a man's head, exceedingly heavy, of a ruſty iron colour, and having a ſtrong ſmell of ſulphur, fell from the heavens with exploſions and a dreadful concuſſion of the air at Miſcoz in Tranſylvania, in the

* In Averrhoes Meteor. lib. ii. cap. 2.

† De Rerum Varietate, lib. xiv. c. 72.

‡ De Subtilitate Exercit. 323.

§ Eſſai de Phyſique, tom. ii. ſect. 1557.

‖ Lettere fiſico-meteorologiche, p. 120.

year 1559, and are preſerved in the treaſury at Vienna. On the 26th of July 1581, between the hours of one and two in the afternoon, a ſtone 39 pounds in weight, of a blue and browniſh colour, and which ſtruck fire with ſteel, fell from the clouds in Thuringia, with an exploſion which ſhook the earth, and accompanied by the appearance of a ſmall light cloud, that could have been nothing but a fire-ball, the heavens at that time being in other reſpects ſerene. This ſtone, which ſunk to a conſiderable depth in the ground, made the earth fly up to twice the height of a man, and was ſo hot that no perſon could touch it. We are told that it was carried to Dreſden. On the 6th of March 1636, while the heavens were perfectly ſerene, a large ſtone fell with a loud craſh between Sagan and the village of Dubrow in Sileſia. It had internally the appearance of a mineral, could be eaſily rubbed to powder, was covered on the outſide with a cruſt, and ſeemed as if burnt by fire. On the 16th of March 1698, a black ſtone fell from the atmoſphere with various exploſions, and was tranſmitted with an account of the circumſtance to the library at Berne. This ſtone is mentioned by Scheuchzer in his Natural Hiſtory of Swiſſerland *. D. Roſt relates, in the Breſlaw Collection reſpecting Natural Hiſtory †, that at two o'clock in the afternoon on the 22d of July 1723, the weather being then ſerene, there was ſeen a ſmall cloud (probably a fire-ball), and at the ſame time ſeveral large and ſmall ſtones fell from the heavens, accompanied by loud exploſions, but without any lightning. Theſe ſtones, which were black on the outſide, had internally the appearance of metal, and exhaled a ſtrong ſmell of ſulphur. It is worthy of remark, that the iron maſs mentioned by Pallas, which has no affinity with any of the known foſſils, but which correſponds in many reſpects with ſome of the before-mentioned maſſes, and particularly that which fell at Agram, was conſidered by the inhabitants of Siberia, where it was found, as a ſacred relic dropped from

* P. II. ad ann. 1726, p. 75. † Page 44.

heaven. It is a pity that the place was not dug up where the fire-ball, mentioned in the Philoſophical Tranſactions *, fell in Jamaica, in the year 1700. In that ſpot there were found ſeveral holes in the earth, which were ſo deep, that the poles with which they were examined did not reach to the bottom of them. A ſulphureous vapor was perceived in the neighbourhood, and the graſs around the holes appeared ſcorched.

When all theſe data, which correſpond ſo well with each other, are compared with the obſervations made on other fire-balls, where no opportunity occurred of getting poſſeſſion of the fallen maſſes, the following concluſions may be drawn:

I. That the accounts given of ſcoriaceous maſſes, which contained iron, earth, ſulphur, &c. having fallen from the heavens with violent exploſions, are not fictions, but true relations of real natural phenomena actually obſerved at various times.

II. That fire-balls and the falling of ſuch maſſes are the ſame meteor.

Reſpecting the queſtion, whence fire-balls and ſuch fallen maſſes proceed, opinions are very different. Moſt people believe that they are owing to accumulations in the atmoſphere. But even when it is allowed that a great many foreign ſubſtances are diſſolved in the atmoſphere, the quantity of them, eſpecially in regions at the diſtance of eighty miles or more, from which ſuch fire-balls are ſeen to fall in the form of a luminous point, is too ſmall to admit of our ſuppoſing ſuch large maſſes to be formed of it. Should the ſolid particles, which may perhaps be diſſolved in the atmoſphere, precipitate themſelves, it would be rather in the form of a fine powder. I conſider it, therefore, with Anaxagoras, Maſkelyne, Halley, &c. as more probable that theſe maſſes come to our regions from the common expanſe of the univerſe, and that, beſides planetary bodies, there are ſmaller

* N° cccl vii. p. 148.

accumulations of matter, which when they approach too near our earth muſt fall down. That material bodies actually exiſt in the remoteſt regions is ſhewn both by the ſingle and accumulated luminous ſparks which Dr. Schröter ſaw paſs over the field of his teleſcope, as alſo by the ſhooting ſtars which paſs by our earth, probably at a greater diſtance and with greater velocity, than to allow their being attracted by it, and made to fall to its ſurface, and to which, fire-balls on their firſt appearance, when they ſeem to approach like a luminous point, have a perfect reſemblance. There are many reaſons for inducing us to believe that ſhooting ſtars cannot be mere electric phenomena without the preſence of ſome coarſer ſubſtances.

The paradoxicalneſs of this mode of explanation, which is contrary to no known obſervations of nature, is rather apparent than real, and conſiſts only in this, that people have not been accuſtomed to it, or that, on account of the rarity of theſe phenomena, many facts of this kind have been denied, or have eſcaped notice. For this reaſon, after I had written the Treatiſe on the Maſs of Iron diſcovered by Profeſſor Pallas, I heſitated whether I ſhould publiſh it, becauſe I expected that it would meet with conſiderable oppoſition. The more I endeavoured however to compare, without partiality for any ſyſtem, the obſervations already made, which correſpond ſo much with each other, the more I found that theſe phenomena could not properly be explained in any other manner, without either contradicting obſervations already made, or well-known laws of nature; ſo that I ſee no grounds for retracting any thing I have advanced on this ſubject.

II. *Hiſtorical Remarks relative to the Manufactures of Iron and Steel in Great Britain. By Mr.* DAVID MUSHET, *of the Clyde Iron Works. Communicated by the Author.*

IT is uncertain at what period the manufacture of iron commenced in Britain. It is probable that the working of the iron mines of Cornwall by the Phœnicians, would introduce into the country a claſs of men ſkilled in all the then known metallic ores; capable of appreciating their true value, by converting the riches of an unexplored country, either to their own immediate wants, or to the conveniencies of the unſkilful inhabitants. The invaſion of England by the Danes, and their conſequent eſtabliſhment, would, moſt likely, add to the former ſtock of knowledge in the art of mining and fuſing ores. Large heaps of ſcoria are to this day met with in many places in England, with ſo great an accumulation of ſoil as to grow trees of a large ſize *; theſe heaps are called Danes cinders, and are in our times ſmelted to advantage for the production of crude iron. From whatever period the iron trade may date its origin; certain it is, that towards the end of the 16th and early in the 17th centuries we find it had attained a pitch of magnitude almoſt incredible, at a time ſo hoſtile to the peaceable views and induſtry of the manufacturer. Hence we find that cannon and mortars, of various calibres and conſtructions, were fabricated from caſt-iron at ſome of the Engliſh works; and that this ſpecies of manufacture was in conſiderable requeſt on the continent. The calibre of the gun was in thoſe days, and within theſe forty years, formed by a loam core of the exact diameter placed vertically in the middle of the mould. The boring and turning mills were quite unknown, together with all that ſcrupulous exactitude which

* Dudley mentions that as early as 1620, oaks were found of a large ſize decayed on the tops of large hills of cinder.

diſtinguiſhes

diſtinguiſhes the artilleriſt, and the manufacture of the preſent times.

Hitherto caſt and malleable iron were obtained from ores with the charcoal of wood, and the manufacture of theſe articles had acquired certainty and extent.

If credit could be given to the *metallum marks* of Dudley, in the 12th year of James, anno 1615, there were at that period not leſs than 300 blaſt furnaces for ſmelting iron ore with charcoal; each of which had fuel, upon an average, for 40 weeks per annum. The average produce in crude iron at each furnace, 13 tons per week, makes the total annual quantity 180,000 tons *; for the production of this aſtoniſhing quantity, calculating at the rate of two loads of charcoal for each ton of iron, and reckoning 8 cwt. charcoal to each load, we find the amazing quantity of 144,000 tons of charcoal neceſſary. As wood in charring nearly affords bulk for bulk, and as a cubic foot will be found to weigh on an average nearly 18·75 pounds avoirdupoiſe, we find that 17,310,000 cubical feet of timber was neceſſary to produce this quantity of charcoal. Again, let this quantity of charcoal be valued at the preſent price (in Scotland), viz. 4*l.* per ton, its value will amount to 576,000 *l.* a ſum little ſhort of the annual value of all the pig iron manufactured at this time in England, Scotland and Wales.

Mr. Dudley alſo informs us, that at the ſame period there were reckoned 500 forges and iron mills for refining the crude iron, and making it into malleable bars. Theſe on an average made 3 tons each per week, for 50 weeks annually = 150 × 500, annual produce in bar iron 75,000 tons; a produce little ſhort of the whole annual export of the Ruſſian and Swediſh markets. Three loads of charcoal were requiſite to refine metal for 1 ton of bar iron; ſo that, if the above calculation of 8 cwt. to each load is adopted, a further

* Upwards of 80,000 tons more than is at preſent manufactured in Britain with pit-coal.

quantity

quantity of 90,000 tons of charcoal would be requiſite for a year's ſupply at that period = 10,732,000 cubical feet *.

Let the waſte of the refining furnace in thoſe days be reckoned equal to 50 per cent. 112,500 tons of pig iron muſt have then been uſed to fabricate 75,000 tons of bars, and a reverſion of 67,500 tons caſt into cannon, mortars, wares, &c. or exported in a raw ſtate as pig iron or ſhip ballaſt.

However much the above quantities may be exaggerated, as I am inclined to think they are, yet they ſeem to prove that, at the commencement of the laſt century, the iron buſineſs had obtained an eminent rank among the manufactures of the country. The progreſs of agriculture and the increaſe of population under the reign of the peaceable James had at length taught the huſbandman and the proprietor the value of cultivated fields. The great conſumption of wood for the navy and iron works had greatly exhauſted the principal foreſts of ſupply: tracts of country became cleared, and in proportion as the ſpirit of cultivation increaſed, the annual quantity of fuel for the manufacturing of iron diminiſhed.

Pit-coal had been long known before this period, and wrought at Newcaſtle prior to the year 1272. Annually vaſt quantities of it were exported to Holland, and the Low

† The aggregate quantity of timber neceſſary for the manufacture of iron alone amounts to 28,062,000 cubical feet. Let an acre of ground be ſuppoſed to afford 2000 cubical feet of timber, then it will be found that 14,031 acres of land were annually ſwept to ſupply the iron manufacturers. Admitting that wood fully replaces its cubical contents in 18 years, then 252,558 acres of land would have been required to furniſh the neceſſary conſumption of timber, without in the end diminiſhing the ſupply. Beſides iron works, ſmiths' and nailors' fires, manufactories of every ſort were carried on by means of wood; even at a time when pit coal was exporting to other countries. The data on which I have made this calculation are moderate. I have ſuppoſed the cube of charcoal produced from the ſame cube of timber; which is not ſtrictly the caſe, as ſome woods ſhrink conſiderably. I have likewiſe reckoned an acre to produce 2000 cubical feet, which I ſuppoſe wood in a natural ſtate ſeldom does, taking it upon an average of the whole country.

Countries,

Countries, for the uſe of the ſmithy, and other manufacturers requiring an intenſe and continued heat. Yet in England prejudices ran ſo ſtrong againſt its application to the manufacturing of caſt or malleable iron, that the projectors of this uſeful undertaking met with every obſtacle which the narrow, unenlightened minds of the eſtabliſhed manufacturers could deviſe.

James granted ſeveral patents for the excluſive right of manufacturing iron with pit-coal. None of the projectors, however, were ſucceſsful, till 1619, when Dudley ſucceeded in making coak pig iron, though only at the ſparing rate of 3 tons per week. By this time many of the iron works were at a ſtand for want of wood; the conſequence was, an advance on the price of iron: this therefore rendered it a lucrative buſineſs to thoſe manufacturers whoſe ſupply of wood was ſtill undiminiſhed, and, of courſe, made them hoſtile to any innovation whereby the preſent price of iron was likely to meet with a reduction.

This period of prejudice ſo unfavourable to innovation in the iron buſineſs, was followed by one more general, and more calamitous for the nation. Amidſt the diſtraction occaſioned by civil war, neither innovation nor improvement could be expected. Patents however were granted to ſome during the commonwealth for the excluſive manufacture of iron in the new way; in one of which it was believed Cromwell was a partner: theſe partly ſhared the ſame fate as did their predeceſſors, and none ſucceeded in eſtabliſhing a manufactory either of extent or of certainty. In 1663 we find Dudley applying for his laſt patent, and ſetting forth, that at one time he was capable of producing 7 tons coak pig iron weekly, with an improved furnace 27 feet ſquare, and bellows which *one man could work for an hour without being much tired.*

It was not till impelled by neceſſity, by the rapid decline of the annual growth of timber, that pit-coal became an object of univerſal eſtimation. When the improvements on machinery

machinery had attained a pitch of certainty, and experience had taught the mechanic the manifold advantages to be derived from the ſteam-engine, men of induſtry and enterpriſe began to think of extending the manufactures of the country in iron with pit-coal. Small furnaces ſupplied with air from leathern bellows, blown by oxen, horſe, or human labour, became exploded, and an increaſe of ſize took place, together with an increaſe of the column of blaſt neceſſary to excite combuſtion. But as it ſeldom happened, that to the advantage of having pit-coal, ores, and limeſtone concentrated in one ſpot, water alſo was added, it became neceſſary to form a ſubſtitute. For this purpoſe, the ſteam-engine, that ſuperhuman invention, was applied. Rude and unpoliſhed no doubt were its early deſigns and execution—rapid however have been its improvements; and at the preſent time, by many it is believed to have arrived at the higheſt poſſible pitch of human perfection.

With the improvements in machinery the advancement in the practice of manufacturing coak pig iron kept pace; and it is now a certain truth, that with pit-coal in our time we produce a quality of pig iron ſuperior for every purpoſe of the arts (bar iron making excepted) to that at any time made with charcoal of wood, and in the following increaſed proportion:

	Tons.
Average annual produce of a charcoal furnace 180 years ago	600
Some furnaces in England on an average now produce 40 tons per week	2080
Value of the former when manufactured £.6 per ton	£.3600
Value of the latter on an average of the qualities 6*l.* 10*s.*	£.13520

By comparing the value of pig and bar iron at preſent with what it ſold for 180 years ago, an accurate notion may be formed of the increaſed price of labour and materials attached to iron works.

About

	£.	s.
About the year 1620 charcoal pig iron ſold for per ton	6	0
In the year 1792, carbonated pig iron -	8	10
In the year 1798 ditto, ditto - -	10	0
Coak pig iron when invented ſold at - -	4	0
In the year 1792 melting pig iron ſold for -	5	10
In the year 1798 ditto - - -	7	10
Malleable iron made with charcoal ſold for - -	15	0
The ſame in 1792, to be drawn into wire, for -	23	0
Ditto in 1798 - - - £.27 or	28	0
The firſt bar iron made (1620) with pit-coal ſold for	12	0
The ſame iron in 1792 ſold for - -	18	0
Ditto in 1798 for - - -	22	0

By theſe ſtatements it may be ſeen that all along there has been preſerved an analogy between the value of the reſpective ſtates of the metal. We cannot, however, be but aſtoniſhed at the mighty advance on iron within the laſt ſix years, nearly and in ſome caſes more than equal to the advance of a period of 170 years before.

Let it be taken for granted, that the manufacture of England, Scotland and Ireland at the beginning of the 17th century amounted in crude iron to 180,000 tons.

112500 tons of which, ſuppoſe, produced 75000 tons bars at 15*l.* Amount -	£.1,125,000
67500 tons caſt into guns, mortars, ſhips' ballaſt, &c. &c. at 10*l.* - -	675,000
180000 tons, amount of the manufactures of iron at that early period - -	£.1,800,000

In Britain at preſent the total produce in pig iron does not exceed 100,000 tons; and reckoning on an average that 33 cwt. of crude iron produces 1 ton of bars, and that the manufacture of malleable iron amounts to 35,000 tons per annum,

57750 tons of crude iron will then be neceſſary to form 35000 tons of bars at 20*l.* £.700,000

42250 tons caſt into cannon, cylinders, machinery wares, &c. at 14*l.* - - 591,500

100000 tons amount of the native manufacture of iron at this period - - £.1,291,500

The extenſive manufacturers of this country have for many years paſt demanded an additional ſupply of foreign bar iron. This has been chiefly received from Ruſſia and Sweden, and the annual quantity may be averaged for the laſt 20 years at 70000 tons, at 18*l.* per ton, - £.1,260,000

£.2,551,500

This may be taken as the annual amount of one raw material, the chief part of which becomes more valuable in an uncommon ratio, by ſubſequent labour.

Amount of the native manufacture at the beginning of laſt century, when the raw material was exported in quantity - - 1,800,000

Balance in favour of this period - - £.751,500

excluſive of the additional value ſtamped upon iron in the now extenſive operations of ſlitting it into rod iron for nails, rolling it into hoops, and converting it into ſteel. In theſe manipulations value is gained as follows: Rod iron 3*l.* 10*s.* per ton. Hoops 7*l.* Bliſtered ſteel 7*l.* to 9*l.* Tilted ſteel 10*l.* to 12*l.* and German ſteel 25*l.* to 28*l.* per ton. Caſt-ſteel according to the ſize of the bars 30*l.* to 45*l.* per ton. Some of theſe operations are attended with a waſte of metal, though not nearly in the proportion of making bar iron from pig iron. The manufacturer of ſteel alſo buys his iron at the

the rate of 20 cwt. to a ton; but in ſelling ſteel he gives at the rate of 21 cwt. 1 qr. 20 lb. to each ton, or 120 lb. inſtead of 112 lb. for each hundred weight.

It is uncertain whether the quality of ſteel, fabricated from Engliſh iron at the above early period, was in any way comparable with what we now make from foreign iron; or whether the artiſts were ſupplied with this ſtate of the metal from Spain and the Low Countries. The latter is moſt probable.

In whatever point of view the iron trade may be conſidered with regard to this country, the advantages derived from its progreſs have been great: whether we conſider it as having cleared the country of vaſt tracts of wood—affording at the ſame time an ample indemnification for the labour beſtowed—the conſequent improvement of climate, and the ſpread of agriculture; as having placed us at the head of the manufacturing countries of Europe; as affording us at all times a plentiful ſupply for the conſtruction of every ſpecies of machinery; as being an arſenal for the rearing and protecting an extenſive navy; or, as having been a ſource of wealth to many individuals, and at the ſame time affording a competent recompenſe for the labour of a number of our fellow creatures.

III. *Account of Count* RUMFORD*'s Experiments on the conducting Power of Liquids with Regard to Heat, and a Deſcription of the Apparatus employed in the Experiments. Extracted from his* Eſſays, Political, Economical and Philoſophical, *Vol. II.*

OUR limits will not allow us to make long extracts from the intereſting experiments of this ingenious philoſopher, who has done more in what regards the ſcience of heat than all who have ever written upon it beſides. We only hope to be able to convey ſome intelligible ideas on the ſubject to

ſuch of our readers as have not ſeen the Count's Eſſays, or who, from their numerous avocations, cannot ſpare time to read a more voluminous account.

In the courſe of a ſet of experiments, in which the Count had occaſion to uſe thermometers of an uncommon ſize, (their bulbs being above 4 inches in diameter) having expoſed one, filled with ſpirit of wine, in a window, to cool after being heated, he obſerved the whole maſs of liquid in a moſt rapid motion, running ſwiftly in two oppoſite directions *up* and *down* at the ſame time. This motion was rendered viſible by ſome particles of duſt which had got by accident among the ſpirit of wine, the ſun happening to ſhine upon the window, as duſt in the air of a darkened room is viſible by the ſun-beams coming through a hole in the window-ſhutter. The aſcending current occupied the *axis of the tube*, the deſcending *the ſides*. On inclining the tube a little, the former moved out of the axis and occupied that ſide which was uppermoſt, the latter the whole of the lower ſide. Quick cooling, by applying ice cold water to the tube, increaſed the velocities of the currents, which, however, ceaſed entirely when the thermometer had acquired the temperature of the room.

Being perſuaded that this motion in fluids, (for the ſame experiment was tried with the ſame reſult with a ſimilar thermometer filled with linſeed-oil*) was occaſioned by their particles *going individually* and *in ſucceſſion* to give off their heat to the cold ſides of the tube, the Count was led to conclude, that fluids are in fact *non-conductors* of heat; and that, if heat be propagated in them *only* in conſequence of the internal motion of the particles, whatever could ob-

* The ſame motions were rendered viſible in water by mixing powdered amber with it, which is nearly of the ſame ſpecific gravity, being 1·078, while that of the former is 1·000. Potaſh was added to the water till it became of the ſame gravity with the amber, the particles of which then remained ſtationary in any part of the liquid, unleſs when the particles of the latter were put in motion by any ſudden change of temperature.

ſtruct

ſtruct thoſe motions would retard that effect. To determine this point, a certain quantity of heat was made to paſs through a given quantity of pure water; and, noting the time employed, the experiment was repeated with water mixed with ſome fine ſubſtance, as eider-down, which, without altering its chemical properties, or impairing its fluidity, could only ſerve to obſtruct and embarraſs the motions of the particles of the water in tranſporting the heat, ſhould heat be *tranſported* and *carried* in this manner, and not paſs freely through it.

The body which received the heat, and which ſerved at the ſame time to meaſure the quantity of it communicated, was a very large thermometer *c f* (Plate IX. *fig.* 1.), with a cylindrical bulb *c* made of thin copper, ſpherical at the ends, 1·84 inches in diameter, 4·99 long, of 13·2099 cubic inches capacity, and meaſuring externally 28·834 ſquare inches: it weighed 1846 gr. and could contain 3344 gr. of water at the temperature of 55°. To this bulb was affixed a glaſs tube *e f* (repreſented in the plate as broken off at *f*) 24 inches long and $\frac{4}{10}$ of an inch in diameter, by means of a cork fitted into a neck of copper 1 inch long, belonging to the bulb. This thermometer, filled with linſeed oil and graduated, was fixed in the axis of a hollow cylinder (*a b* is a ſection of it) made of thin ſheet-copper $11\frac{1}{2}$ inches long, and 2·3435 inches diameter within, with a ſpherical bottom, weighing 2261 gr. The bulb of the thermometer was made to occupy the lower part of this cylinder by means of 4 pins of wood $\frac{1}{10}$ of an inch in diameter and $\frac{1}{4}$ of an inch long, one of them faſtened to the bottom, the other three round the inſide of the cylinder at equal diſtances, in very ſmall ſockets made to receive them. (Theſe pins may be ſeen at the lower part of *fig.* 1. and alſo in *fig.* 2. which repreſents a horizontal ſection of the cylinder and a bird's-eye view of the thermometer in its place.) The diſtance between the external ſurface of the bulb of the thermometer and the internal of the containing cylinder was 0·25175 of

an inch, and could contain, at the height of $\frac{1}{4}$ of an inch above the bulb, 2468 grains of water.

The bulb of the thermometer being ſurrounded by water, or by any other liquid, or mixture, the conducting power of which was to be aſcertained, a cylinder of cork *h* ſomething leſs * in diameter than the braſs cylinder, about half an inch long, having a hole in its centre in which the tube of the thermometer paſſed freely, was thruſt down into the braſs cylinder nearly to the ſurface of the ſubſtance it contained, and made to reſt on three projecting braſs points fixed to the neck of the copper bulb. The upper part of the tube was then filled with eider-down and cloſed with a cork ſtopper *g*, the tube of the thermometer, which paſſes through a fit hole in the middle of this ſtopper, projecting upwards. The ſcale from the point of freezing to that of boiling water is on that part of the tube which riſes above the ſtopper, and is divided according to that of Fahrenheit.

The thermometer being fixed in the tube as above deſcribed, ſurrounded by the ſubſtance the conducting power of which was to be aſcertained, the inſtrument was placed in thawing ice, and kept there till the thermometer fell to 32°. It was then taken out and inſtantly plunged into boiling water, and the conducting power of the ſubſtance under experiment was eſtimated by the time required to make the heat paſs through it into the thermometer; the time being carefully noted when the liquid in the thermometer arrived at 40°, and alſo when it came to every 20th degree above it.

A number of experiments were made with this inſtrument, all of which tended to prove that the paſſage of heat through water is much impeded, by mixing other ſubſtances with it, whether they be ſuch (eider-down for inſtance) as merely

* If this cork had been made to fit cloſe, yet ſo as to move in the braſs cylinder, and its centre hole been well fitted to the glaſs tube, the trouble of contriving and fixing pins to keep the bulb in its place might have been avoided. EDIT.

embarraſs

embarraſs the water in its motions, or thoſe (mucilages, &c.) which leſſen its fluidity.

The time required to heat the inſtrument from 32° to 200, when the bulb of the thermometer was ſurrounded with

1. 2276 gr. of water and 192 gr. of ſtarch, was 1109″
2. 2276 gr. of water and 192 gr. of eider-down, 949
3. With water and 48 gr. or $\frac{1}{30}$th of its bulk of eider-down - - - - 763
4. With ſtewed apples - - - 1096$\frac{1}{2}$
5. With pure water - - - 597

The ſtarch was boiled with the water with which it was mixed, as was alſo the eider-down—the latter to free it from air, which adheres to it with great obſtinacy.

As Count Rumford ſuppoſes, and juſtly, that there is a poſſibility of error when the reſults are determined by the extreme points *freezing* and *boiling*, owing to the ſlowneſs of the proceſs when the inſtrument approaches the medium of the temperature in which it is placed, he thinks they may be got more accurately by taking the times occupied in heating from 80° to 160. They were as follows:

with the 1ſt	341″
with the 2d	269
with the 3d	215
with the 4th	335
with pure water	172.

The time the heat took to paſs out of the thermometer through the ſame ſubſtances

	from 200° to 40°	from 160° to 80°.
through the 1ſt	1548″	468″
the 2d	1541	460
the 3d	1395	373
the 4th	1749$\frac{1}{2}$	520
Water	1032	277

As the reſults of theſe experiments prove that the propagation of heat in water is retarded, not only by thoſe things which diminiſh its fluidity, but alſo by thoſe which, by me-

chanical means, and without forming any combination with it whatever, merely obſtruct its internal motions, it appears to the ingenious author, and we believe it will not be denied, that this proves, that heat is propagated in water *in conſequence* of its internal motions;—or, that it is tranſported or *carried* by the particles of that liquid, and that it does not ſpread and expand in it, as in metals and other conductors of heat, as has generally been imagined.

The ſucceſs of theſe experiments encouraged the author to plan and execute others ſtill more deciſive and extremely intereſting. It has been generally believed that water cannot be heated in contact with ice: reflecting on the ſubject, he perceived that either this muſt be a miſtake, or his ideas reſpecting the manner in which heat is propagated in that fluid, erroneous. On the ſuppoſition that water is not a conductor of heat, according to the common acceptation of that term, or that heat cannot paſs in that fluid, except when *carried* by its particles, which, being put in motion by the change it occaſions in their ſpecific gravity, tranſports it from place to place, it did not appear how ice, if, inſtead of being permitted to ſwim on water, it were confined at the bottom of it, or any where below the ſurface, could affect the temperature of the ſuperincumbent water, or prevent its receiving heat from other bodies:——but, on the other hand, the ice-cold water which reſults from the melting of ice when on the ſurface of that fluid, cannot but deſcend by its gravity to the bottom of the containing veſſel in an uninterrupted ſtream; ſo that as long as this operation is going on the maſs of water cannot be much heated.

A cylindrical glaſs jar 4·7 inches diameter and 14 inches high was nearly filled with boiling hot water. A circular cake of ice of nearly the ſame diameter, 3′ inches thick, and weighing 10½ oz. was then gently put on the ſurface of the water. In 2′ 58″ it was entirely melted.

A cake of ice of the ſame form and dimenſions with the above was afterwards faſtened down on the bottom of the

ſame

ſame jar, by means of two ſlender laths, a trifle longer than the diameter of the jar, forced down into it, and laid acroſs each other over the ice. Cold water to cover the ice to the height of $\frac{1}{4}$ of an inch was then poured into the jar to prevent its breaking by the ſudden application of boiling water with which it was next filled. To protect the ice while the hot water was pouring in, a circular piece of paper was previouſly laid on its ſurface, which was afterwards gently removed by means of a ſtring faſtened to one ſide of it. The reſult of the experiment proved, that the Count was not miſtaken in his ideas. When 20′ had elapſed, the heat in the water at different depths was found to be—immediately above the ſurface of the ice 40°; at $\frac{1}{2}$ inch above it 46°; at 1 inch 130°; at 3 inches 159°; at 7 inches 160°; at the end of 35′ it was, at the ſurface of the ice 40°; at $\frac{1}{2}$ inch 76°; at 1 inch 110°; at 2 inches 144°; at 3 — 148°; at 5 — 148$\frac{1}{2}$°; at 7 — 149°. After an hour and 15′ it was at the ice 40°; 1 inch above it 82°; 2 inches 106°; 3 — 123°.—Theſe were taken near the ſide of the jar, the following at the axis: at the end of 1 h. 30 m. at the ſurface of the ice 40°; at 1 inch above it 84°; at 2 inches 115°; at 3 — 116°; at 7 — 117°. At the end of 2 hours ſtill 40° at the ſurface of the ice; 76° at 1 inch above it; 94° at 2 inches; 106° at 3; 108 at 4; 108$\frac{1}{4}$° at 6; 108$\frac{1}{2}$ at 7 inches.—An end was now put to the experiment, the water was poured off; and the remaining ice being weighed, it was found that 5 oz. 6 gr. Troy had been melted. Taking the mean temperature of the water at the end of the experiment at 106°, it appeared that 73$\frac{1}{4}$ oz. of hot water employed was cooled 78°. As 1 oz. of ice abſorbs juſt as much heat in being melted as 1 oz. of water loſes in being cooled 140°, *each* of theſe 73$\frac{1}{4}$ oz. would give off as much heat as would melt $\frac{78}{140}$ths of 1 oz. of ice; that is, all of them together as much as would melt 40$\frac{8}{10}$ ounces of ice; but the quantity melted was only about 5 oz. and hence it appears that *leſs than* $\frac{1}{8}$$\frac{}{0}$th *part of the heat loſt by the water was communicated to the air.* Or, *ice melts*

eighty times ſlower at the bottom of a maſs of boiling-hot water than when ſwimming on its ſurface.

But the ice was melted, though ſlowly, at the bottom of the hot water, which the author accounts for in a ſatisfactory manner without needing to abandon his hypotheſis. Freſh water preſents an exception to the general law of condenſation by cold: it condenſes in cooling till it comes to 40°, but on cooling it lower *it expands*, and continues to expand till it freezes and after it has become ice *. Water in contact with melting ice is always at the temperature of 32°, and therefore ſpecifically lighter than that which is 8 degrees warmer, or at 140°: the latter will therefore *deſcend*, being heavier, and by its greater heat melt a portion of the ice, be cooled, and then give place to the deſcending currents of warmer water which ſucceed it. It appears too from the experiments which followed, that a conſiderable portion of the ice that was melted, was melted in conſequence of the motions into which the water was thrown on being poured into the jar.

The experiments on which the author builds his reſults were conducted in the following manner: 43·87 cubic inches, or 1 lb. 11½ oz. Troy of water was put into the jar *e f c d*, *fig.* 3, which was placed in an earthen bowl and ſurrounded with a freezing mixture compoſed of ice and common ſea ſalt, by which means the water in the jar was frozen into one compact maſs adhering to its bottom and ſides. The jar was then removed from the freezing mixture, and placed in a mixture of pounded ice and water for 4 h. that the cake of ice might be brought to the temperature of 32°, when the ſurface of the ice, *c d*, was covered to the height of 0·478 of an inch with ice cold water before pouring in the boiling water. Inſtead of a piece of paper as before, a wooden diſh perforated with many hundreds of

* Salt water on the contrary continues to be condenſed the more, the more it is cooled.

holes

holes was put on the ſurface of the ice to defend it while the hot water was pouring in, which was done through a long wooden tube, ſtopt with a cork at the lower end, but perforated with a number of ſmall holes in the ſides, immediately above the cork, to make the water iſſue horizontally, and not impinge with force againſt the bottom of the wooden diſh, which of courſe floated as the water roſe; which tended to leſſen the motions in the liquid. The diſh was then carefully removed from the ſurface of the water *e f*.

The experiments were varied by ſometimes leaving the upper part of the jar ſurrounded by the atmoſphere, at other times by covering the part occupied by the hot water with cotton, and, at others, by keeping the jar plunged to its brim in melting ice and water.

The mean quantity of ice melted by hot water in 30′, when the jar was kept plunged to its brim in melting ice and water, was $399\frac{2}{5}$ grains; when ſurrounded by air at the temperature of 41°, 456 gr.; when ſurrounded by air at the temperature of 61°, $558\frac{1}{2}$ gr.; when covered up by a thick and warm covering of cotton, $690\frac{1}{2}$ gr.

When the experiments were ſimilar, the mean reſults of thoſe which were made with water at different temperatures were as follows:

In the experiments in which the part of the jar occupied by the water was expoſed uncovered to the air at the temperature of 61°, with boiling water the quantity of ice melted in 30 minutes was $558\frac{1}{2}$ gr.; with water at the temperature of 61°, 646 gr.; with water at the temperature of 41°, 574 gr.

In the experiments in which the jar was wholly ſurrounded by pounded ice and water, and conſequently was at 32°, the quantity melted in 30′ with boiling-hot water was $399\frac{2}{5}$ gr.; with water at the temperature of 61°, 661 gr.; with water at the temperature of 41°, 541 gr.

From

From these results the ingenious author concludes, and certainly his experiments justify the conclusion, that boiling-hot water is not capable of melting more ice, *when standing on its surface*, than an equal quantity of water at the temperature of 41°, or when only 9 degrees above freezing—a fact which proves that water is a perfect non-conductor of heat, and that heat is propagated in it *only* in consequence of the motions which the heat occasions in the insulated and solitary particles of that fluid, by altering their specific gravities. He here observes, in a note, that the insight which this discovery gives us in regard to the nature of the mechanical process which takes place in chemical solutions, is too evident to require illustration.

The author having frequently observed, when freezing water in one of his jars into a cake of ice, by placing the jar in a freezing mixture, that the water in the axis of the jar, which was latest in freezing, being compressed by the expansion of the ice round it, was always forced upwards, and formed a pointed projection of ice in the form of a nipple, which was sometimes above $\frac{1}{2}$ inch high, was led by the circumstance to make another experiment, which proved oil to be also a non-conductor of heat.

Into a jar (see *fig.* 4) containing a cake of ice *a* of the form above described, some fine olive oil, previously cooled to 32°, was poured till it stood (at *b*) 3 inches above the surface of the ice. A solid cylinder of iron *c* $1\frac{1}{2}$ inch in diameter, and 12 inches high, and a paper sheath over it, the lower end of which projected $\frac{1}{10}$ of an inch past the cylinder, so contrived as to be suspended over the centre of the ice by means of the hook *d* and the wire *e*, fastened to the ceiling of the room, being heated to 210° in boiling water, was immersed in the oil so deep that the middle of the flat surface of the lower end of the hot iron was only distant from the conical projection of the ice $\frac{2}{10}$ of an inch. Had any heat *descended*, a portion of the ice would have been

been melted, but it was no way affected by the vicinity of the hot iron. The intention in making the ſheath project $\frac{1}{10}$ below the cylinder was to prevent internal motions in the oil, the particles of which, by becoming ſpecifically lighter by being in contact with the iron cylinder, would otherwiſe have ſlid from under it and aſcended.

A ſimilar experiment was tried with mercury poured to the height of 1 inch over the ice, inſtead of the 3 inches of oil, and the reſult was exactly the ſame; though the ice at the time could not bear a finger to touch it (by putting it down through the mercury), however expeditiouſly removed, without a portion being diſſolved, and which inſtantly became viſible on the bright ſurface of the mercury.

From his experiments the author ſeems inclined to doubt the exiſtence of that peculiar kind of attraction of predilection which has been called chemical affinity, and to attribute the equal diſtribution of the particles of any body in its menſtruum to the internal motions among the particles of the fluid occaſioned by accidental changes of temperature, produced by the union of the two; or to a great difference in the ſpecific gravity of the menſtruum in its natural ſtate, and that of the ſame fluid after it has been changed to a ſaturated ſolution. The following experiment ſerves to ſupport the author's opinion:

A cylindrical glaſs jar was placed within a larger one which ſtood in a ſhallow earthen diſh, nearly filled with pounded ice and water. Ice cold freſh water, lightly tinged of a red colour with turnſol, was poured into the ſmall jar to the height of 2 inches; and then, by means of a funnel, which ended in a long and narrow tube, put down through that water and reſting on the bottom of the jar, an equal quantity of the ſtrongeſt brine that could be made with ſea-ſalt was poured in, ſo gently that, without diſturbing the freſh water, the brine took the lower ſtation, the coloured water repoſing above it without the ſmalleſt appearance of blending.

blending. The ſpace between the jars was now filled, almoſt to the top, with ice cold water mixed with pieces of ice (pounded ice would have obſtructed the view), and then ice cold olive oil was gently poured on the ſurface of the tinged water to the height of an inch above it. In a temperature of about 36° the brine and water ſhewed not the leaſt diſpoſition to mix during 4 days. At the end of that time it was removed, without agitating its contents, into a warmer room; in leſs than an hour the brine and water began to mix: they were completely mixed in 24 hours, as was then evident by the uniform colour of the aqueous fluid on which the oil repoſed.

Fig. 5 is a repreſentation of the apparatus employed in the proceſs, and ſhews the different liquids repoſing one above another; with the ſpace between the cylindrical veſſels filled with water and pieces of ice, ſo high as to overtop the ſurface of the contents of the inner jar.

IV. *Experiments and Obſervations on the Nature of Sugar, by Mr.* CRUICKSHANK. *Extracted from* Caſes of the Diabetes Mellitus. *Second Edition.* 1798.

SUGAR has been ſuppoſed to be a ſubſtance intermediate between the vegetable mucilages and acids, containing more oxygen than the former, but leſs than the latter. With a view to aſcertain this and ſome other facts relative to theſe ſubſtances, Mr. Cruickſhank made the following experiments:

Two ounces of refined ſugar were expoſed in a retort to a heat gradually increaſed till the bottom became red hot, 8½ dr. of pyromucous acid came over, which required 150 gr. of a ſolution of potaſh to ſaturate it: this liquor contained mixed with it a little empyreumatic oil: the charry reſiduum in the retort weighed 5 dr. Therefore the gas which eſcaped

muſt

muſt have weighed 2½ dr.: ſome of it being examined, was found to conſiſt of carbonic acid gas and hydro-carbonat.

Two ounces of gum arabic treated in the ſame manner yielded 7 dr. 40 gr. pyromucous acid containing a little more empyreumatic oil, and required only 118 gr. of the ſame ſolution of potaſh to ſaturate it; 3 dr. 45 gr. reſiduum; 5 dr. of gas conſiſting like the former of hydro-carbonat and carbonic acid gas.

As ſugar yields more acid than gum in the proportion of 150 to 118—as oxygen is the univerſal acidifying principle—and as the acid from both is the ſame, viz. the pyromucous, ſugar muſt, therefore, contain the greateſt proportion of oxygen; for the carbonic acid and hydro-carbonat were not produced in any quantity till near the end of the operation, and therefore were probably formed by the decompoſition of the water.

To obſerve the changes and decompoſitions that take place during the proceſs of malting, which ſomehow converts vegetable mucilage and fecular into ſugar, the following experiments were made:

Barley ſoaked in water for 24 hours was introduced in a wine glaſs into a jar containing common air, inverted over water and kept in a temperature between 60° and 70°. In 5 days it began to grow, and in 4 weeks the ſhoots were half an inch long. At the end of 10 weeks vegetation was ſtill going on, and the air in the jar had ſomewhat diminiſhed: the barley was very ſweet, and nearly malted. The air was found to conſiſt of azotic and carbonic acid gas, in the proportion of 20 to 6, the whole of the oxygen being either abſorbed or united to carbon.

Barley ſoaked for 48 hours introduced as in the laſt experiment into a jar containing oxygen gas, inverted over water to which ſulphuric acid had been added, began to grow at the end of 3 days. In 10 days the water had riſen in the jar, the gas being diminiſhed about one third. The barley ſmelled like malt, and was ſweet. The gas was found to con-

ſiſt of 64 parts carbonic acid, 32 azote and 4 oxygen; from which it would appear that the oxygen employed had contained originally about 20 per cent. of azotic gas.

The ſame experiment was repeated over mercury. In 3 days the barley began to grow. At the end of 10 days the gas, which continued of the original bulk, was found to conſiſt of carbonic acid gas, containing $\frac{1}{50}$th of its bulk of oxygen gas. The barley was partly converted into malt.

Another experiment with common air was made at the ſame time: the barley did not begin to grow till the end of the 4th, and at the end of the 10th day had made much leſs progreſs than that in the oxygen. The air had increaſed a little, and was found to conſiſt of carbonic acid and azotic gas, in the proportion of 1 to 2, and a very ſmall quantity of oxygen. The barley taſted ſweet.

Soaked barley was introduced into a jar *filled with* and inverted over mercury. At the end of 12 days a very conſiderable quantity of gas, found to be carbonic acid, was produced; but no vegetation had taken place, nor any other ſenſible change in the barley.

Another portion of the ſame ſoaked barley, expoſed to nitrous gas over water, at the end of 10 days had undergone no apparent change. The gas, which was diminiſhed a little, contained about $\frac{1}{5}$th of its bulk of carbonic acid gas: the remainder was pure nitrous gas, as was manifeſt from the diminution it underwent when mixed with pure air. The nitrous gas which diſappeared muſt have been abſorbed either by the barley or the water: the carbonic acid gas which was produced is accounted for by the laſt experiment.

Two other portions were expoſed, the one to hydrogenous and the other to azotic gas. In 14 days no vegetation in either. Both the gaſes had increaſed about $\frac{1}{5}$th in bulk, and contained each from $\frac{1}{3}$d to $\frac{1}{4}$th of carbonic acid, the remainder being the original gaſes not ſenſibly changed. The barley in the hydrogen taſted muſty—no ſweetneſs.

It is manifeſt from theſe experiments that oxygen is abſo-

lutely

lutely neceſſary for the converſion of vegetable mucilage and fecula into ſugar. In no inſtance was ſaccharine matter formed where this was not preſent. Mr. C. thinks that the oxygen is chiefly abſorbed by the barley, though part may alſo go to the formation of the carbonic acid; for carbonic acid was formed in conſiderable quantity without the preſence of oxygen gas, which he conceives muſt proceed from the decompoſition of the water, its oxygen uniting with the carbonaceous principle of the barley, while its hydrogen is fixed, which may be neceſſary to the formation of the ſaccharine principle. He infers, therefore, that vegetable mucilage is converted into ſugar, by being deprived of part of its carbon, while at the ſame time it unites with a greater proportion of oxygen, and probably alſo with hydrogen from the decompoſition of the water. Thus then both from analyſis and ſyntheſis it appears that ſugar contains more oxygen than gum or mucilage; and it ſhould follow, that ſugar deprived of its oxygen muſt loſe its ſweetneſs and form a kind of gum. To accompliſh this was the object of the following experiments:

Some ſyrup was introduced into a jar filled with and inverted over mercury: to this was admitted an equal quantity of the phoſphuret of lime: a conſiderable production of phoſphoric gas almoſt immediately took place, and the mercury deſcended in the jar. At the end of 8 days the ſyrup, inſtead of a ſweet, had a bitter aſtringent taſte. When filtered, alcohol produced a copious white precipitate in flakes, reſembling mucilage ſeparated from water by the ſame ſubſtance.

To refined ſugar diſſolved in alcohol phoſphuret of lime was added: no gas was diſengaged, nor any action produced. The mixture was allowed to remain in an open vial till the alcohol had evaporated; diſtilled water was added, but no diſengagement of gas took place, the phoſphuret having been decompoſed and converted principally into phoſphat of lime. The mixture being filtered and the clear liquor evaporated,

there remained a ſubſtance, extremely tenacious, which had much the appearance of gum arabic: its taſte was bitter with a ſlight degree of ſweetneſs; when ſqueezed between the teeth it had exactly the feel of gum, but more tenacious; it was not ſoluble in alcohol, at leaſt in any quantity; and burned on red hot iron like gum, leaving a bulky and inſipid charcoal.—That this converſion of the ſaccharine principle into a gum was effected by the abſtraction of oxygen, is highly probable from the nature of the ſubſtance employed; for there are few ſubſtances which have a ſtronger affinity for oxygen than phoſphuret of lime.

Solutions of ſugar were mixed with different ſulphurets, and agitated with nitrous gas in cloſe veſſels. The ſulphurets, eſpecially that of potaſh, deſtroyed the ſaccharine taſte; but owing to the ſolubility of the different products, the nature of the change could not be accurately aſcertained. The action of the nitrous gas was more doubtful.

To be ſatisfied how far the effects produced in the former experiments might be owing to the abſtraction of oxygen, Mr. C. added to ſolutions of ſugar in water both lime and pure potaſh, and boiled the mixtures. The lime combined with and gave to the ſugar a bitter aſtringent taſte; but it was ſtill ſweet: alcohol put to the filtered ſolution produced a precipitate in white flakes, ſimilar to that in the experiment with the phoſphuret, and which appeared to be a combination of ſugar with lime. Vitriolic acid likewiſe precipitated the lime in the form of ſelenite, and in a great meaſure reſtored the natural taſte. Some of the filtered ſolution being evaporated by a gentle heat, there remained a ſemitranſparent ſubſtance, much more tenacious than the thickeſt ſyrup, but leſs ſo than that produced by the phoſphuret of lime: it had a rough bitter taſte, with a certain degree of ſweetneſs. The potaſh likewiſe appeared to combine with the ſugar, the ſweet taſte being more completely deſtroyed than by the lime: but on the addition of ſulphuric acid ſulphat of potaſh was formed; and this being precipitated by alcohol,

alcohol, the ſweetneſs was completely reſtored. When alcohol was added to a ſolution of ſugar and pure potaſh, after it had been boiled to the conſiſtence of a ſyrup, no union took place: though agitated, the alcohol ſtill ſwam pure on the top—which ſeems to prove that a new compound is formed by theſe ſubſtances, inſoluble in this fluid, though both are completely ſo in a ſeparate ſtate.

Having converted ſugar into a ſpecies of gum, by abſtracting a portion of its oxygen, ſeveral experiments were made to change gum, by adding oxygen, into a kind of ſugar, but without ſucceſs. The gum went rapidly into the acid ſtate, eſpecially when expoſed to the action of the oxygenated muriatic acid gas. From the failure of theſe trials, it was ſuſpected that mucilages were not ſo ſimple as had been generally ſuppoſed: to throw ſome light on this ſubject the following comparative experiments were made:

An ounce of powdered gum arabic was expoſed to heat in a coated glaſs retort, as in the firſt two experiments, only here the pneumato-chemical apparatus was adapted—the heat was gradually applied as before *. The charry reſiduum burned very ſlowly; but when expoſed to a ſtrong heat in an open crucible, it left a whitiſh powder, which was found to be lime and a ſmall proportion of calcareous phoſphat. The pyromucous acid being ſuperſaturated with lime, a ſtrong ſmell of ammonia was inſtantly perceived; and a piece of paper dipped in muriatic acid being held over the veſſel, copious white fumes were immediately produced—azote therefore forms a conſtituent part of the gum. The gas collected in the pneumatic apparatus was carbonic acid mixed with that ſpecies of hydrocarbonat which is obtained by heat from moiſtened charcoal. Two ounce meaſures of the gas, well freed from carbonic acid, were mixed with $1\frac{1}{2}$ of very pure oxygen gas, and fired, by the electric ſpark, in

* For the amount of the products in this and the ſubſequent experiments ſee the table, page 374.

a ſtrong glaſs jar, inverted over mercury: when fired they were reduced to 1 meaſure, which was all abſorbed by lime water, admitted for the purpoſe, except a ſmall particle which was found from the nitrous teſt to be pure air. From many experiments Mr. Cruickſkank found that 12 meaſures of oxygen gas, united with carbon, produce 10 of carbonic acid gas. Hence it follows, that the quantity of oxygen gas neceſſary to the formation of carbonic acid gas muſt in this caſe have been 1·1 meaſures or a little better; the remaining ·4 therefore were conſumed in the production of water, and would be ſufficient to ſaturate ·8 of hydrogen, equal in weight to ·048 of a gr. nearly.—Now 1 oz. meaſure of carbonic acid gas weighs ·864 of a gr. and this contains ·24 of pure charcoal; hence the quantity of charcoal to hydrogen in this inflammable gas muſt be as ·24 to ·048, or 5 to 1. But one meaſure of pure hydrocarbonat, ſuch as may be obtained from the decompoſition of camphor, by making its vapour paſs through a red hot earthen tube, or from the diſtillation of animal ſubſtances, opium, &c. requires 2 of pure air to ſaturate it, and the quantity of carbonic acid amounts to 1·45, which makes the proportion of carbon to hydrogen as 12 or 13 to 1: hence theſe gaſes differ materially, and ought not to be confounded. Indeed the difference between them is manifeſt from the manner in which they burn when mixed with common air, and brought into contact with an ignited body: pure hydrocarbonat burns ſlowly with a perfectly white flame, and never detonates—on the contrary, the compound inflammable gas, juſt mentioned, burns rapidly with a reddiſh blue flame and more or leſs of a detonation. It was neceſſary to mention this circumſtance, as much confuſion might be produced by applying the ſame name to ſubſtances conſiderably different. It is remarkable that ether, decompoſed by heat, affords pure hydrocarbonat, while alcohol yields the mixed ſpecies.

An ounce of gum tragacanth treated in the ſame manner

as

as the gum arabic, gave the ſame kinds of products; but the quantity of ammonia diſengaged from the pyromucous acid by ſuperſaturating it with lime was conſiderably greater than from that afforded by the gum arabic.

Having thus diſcovered lime in both of theſe gums, reagents were employed to ſee if in this way that earth could be detected. Sulphuric acid was dropped into a ſolution of gum arabic: after ſtanding for ſome hours a number of needle-like cryſtals were depoſited: theſe being ſeparated were re-diſſolved in diſtilled water; the oxalat of ammonia was then added, and a copious precipitation of oxalat of lime immediately took place.

From theſe experiments it is manifeſt that gums conſiſt of oxygen, hydrogen, carbon, azot and lime.

The pyromucous acid, obtained from ſugar by the ſame proceſs of diſtillation which had been employed on the gums, being ſuperſaturated with lime, no ammonia could be perceived—ſugar therefore does not contain azot: neither does it contain lime; for the charcoal, which was of a beautiful black colour, burned out completely when expoſed to a ſtrong red heat.

Sugar of milk being diſtilled in the ſame manner, and the products treated in the way already deſcribed, was found to contain lime in the charry reſiduum; and, contrary to what might have been expected, the quantity of ammonia diſengaged from the pyromucous acid obtained from it was ſo ſmall that it could hardly be detected—It therefore contains hardly any azot.

Experiments were next made to aſcertain the quantity of oxalic baſis or radical contained in each of theſe ſubſtances.

One ounce of ſugar was added to 6 oz. of concentrated nitrous acid diluted with an equal bulk of water: when the action had ceaſed, heat was applied, and the evaporation continued till the liquor was reduced to about 1 oz. by meaſure: when cold the cryſtals were ſeparated by filtration,

and the remaining fluid was again evaporated till the whole when cold ſhot into a maſs of cryſtals, leaving only a few drops which refuſed to cryſtallize.

One ounce of gum arabic was treated in the ſame manner; the laſt cryſtals obtained were mixed with an inſoluble white powder, which was ſeparated by the addition of diſtilled water, and was found to be oxalat of lime.—One ounce of gum tragacanth was alſo ſubjected to the ſame proceſs.

The cryſtals obtained from ſugar of milk by the ſame proceſs were mixed with a white powder, which, being but little ſoluble in water, was readily ſeparated, and appeared to be the ſaccholactic acid of Scheele. (See Phil. Mag. Vol. II. p. 246.)

Honey treated in the ſame manner exhibited no ſigns of ſaccholactic acid.

The Table ſubjoined to the preſent article will ſhew at one view the different products reſulting from deſtructive diſtillation, with their relative proportions; and likewiſe thoſe obtained by the nitrous acid.

From theſe experiments it appears that ſugar conſiſts merely of carbon, hydrogen, and oxygen; that gum differs from ſugar, not only in containing a leſs proportion of oxygen, but, alſo, by its combination with lime and azot; and that the ſugar of milk differs from both, as it contains the radical of the ſaccholactic acid—in other reſpects, however, it approaches very nearly to the nature of vegetable ſugar. Does the milk of carnivorous and graminivorous animals yield the ſame proportion of this acid, and is this ſugar itſelf always of the ſame nature?

From the well known facts reſpecting vinous fermentation, there is now reaſon to ſuppoſe that no ſubſtances but thoſe which conſiſt ſimply of carbon, hydrogen and oxygen are ſuſceptible of it; and that an union with a fourth changes the nature of the compound ſo much as to render this proceſs impracticable. To a ſolution of ſugar digeſted with freſh

freſh ſlacked quick lime, and to another digeſted with potaſh, ſome good yeaſt was added—to an equal quantity of ſolution of ſugar alone the ſame quantity of yeaſt was added: the laſt began to ferment in 12 hours, and in a few days ran into the acetous ſtate—the other two did not ferment: the temperature of the room during the experiments was from 65° to 75°.

A ſolution of the ſugar of milk, to which a proper quantity of yeaſt was added, began to ferment in four days: at the end of 16, when the proceſs had ceaſed, it had the ſmell of cyder, but rather more of the flavour of apples, was very ſour, and reddened the tincture of litmus: with the oxyd of lead it formed a ſweet ſaline maſs compoſed of ſlender priſmatic cryſtals, not deliqueſcent—the acid therefore contained, or was common vinegar.

In the attempts to convert gum into ſugar by combining oxygen with it, as before ſtated, it ran into the acid ſtate. To ſee if any thing like fermentation preceded this ſtate, or if this effect could be produced by mere expoſure to the air, without the addition of ſome ſubſtance containing much oxygen, as the nitrous or oxygenated muriatic acids, a ſolution of gum arabic was mixed with yeaſt as in the laſt experiments, but no fermentation was perceivable at the end of 26 days. It appears that the azot and lime combined with the carbon in gum prevents the vinous, and conſequently the acetous fermentation. Similar experiments were made upon a ſtrong decoction of beef, with, and without yeaſt, but no fermentation followed. Indeed it has been ſo generally admitted that vegetable and animal mucilages, when pure, are not ſuſceptible of the vinous fermentation, that, had not a contrary opinion been lately advanced, theſe experiments upon them would have been conceived unneceſſary.

T A B L E.

Subſtances employed 1 oz. of each.	Products obtained by heat.				Products obtained by nitrous acid.	
	Charcoal, &c.	Pyromucous acid, &c.	Carbonic acid gas.	Hydrocarbonat.	Oxalic acid.	Other ſubſtances.
Sugar - - -	oz. drs. grs. 0 2 0	oz. drs. grs. 0 4 30	oz. meaſures 41	oz. meaſures 119	oz. drs. grs. 0 4 20	None
Honey - - -					0 4 4	None
Sugar of milk -	0 1 0	0 6 0	31	103	0 3 18	30 gr. faccholactic acid
Gum arabic - -	0 1 36 and lime 10 0 1 46	0 3 30 with ſome ammonia	93	180	0 3 30	Oxalat of lime 6 gr.
Gum tragacanth	0 1 33 and lime 12 0 1 45	0 4 5 with ſome ammonia	78	91	0 3 10	Oxalat of lime 10 gr.

V. *On the different Proportions of the Metals employed in the Preparation of ſoft Solder.*

WHEN lead, tin, and biſmuth are mixed in a certain proportion, they produce a metal exceedingly fuſible, which is known by the name of ſoft ſolder; but which, from its ſingular properties, may be applied with advantage to many other uſeful purpoſes. Newton, and after him Kraft and Muſſchenbroek, obſerved, that 5 parts of biſmuth, 3 of tin and 2 of lead, alſo 5 parts of biſmuth, 4 of tin and 1 part of lead, melted with a heat of 220 degrees of Fahrenheit, and they found that various mixtures of this kind were fuſible by a heat not much greater than that of boiling water. At a later period V. Roſe, a German naturaliſt, diſcovered that a mixture of 4 parts of biſmuth, 2 of tin and 2 of lead, as Kunkel recommended for ſoldering tin; and d'Arcet among the French, that a mixture of 8 parts of biſmuth, 3 of tin and 5 of lead; or 8 of biſmuth, 4 of tin and 4 of lead; or 8 of biſmuth, 2 of tin and 6 of lead; alſo 16 of biſmuth, 7 of tin and 9 of lead, all melted, or at leaſt became ſoft, in boiling water.

According to the experiments made by Profeſſor Gmelin, reſpecting the fuſion of theſe three metals, a mixture, ſuch as that before recommended by Homberg to anatomiſts for injecting into the veſſels of their preparations, conſiſting of 2 parts of biſmuth, 1 part of tin and 1 of lead, which is the ſame as Roſe propoſed, gave a metal that was fuſed in boiling water. A mixture of 6 or more parts of biſmuth, 6 of tin and 3 of lead, or 1 part of biſmuth, 2 parts of tin and 2 of lead, gave, according to Klein, the ſolder uſed by the tin-button-makers. The ſame workmen uſe alſo for ſoldering, according to Klein, a mixture of 4 parts of biſmuth, 3 parts of tin and 5 parts of lead. Among the many ſoft ſolders employed by the tin-men, a mixture of one part of biſmuth, 2 parts of tin, and 1 part of lead, is, according to

Klein, very much employed. Respecting this kind of solder, the experiments of Professor Gmelin give the following result: one part of bismuth, 2 parts of tin, and 1 part of lead, melt in boiling water. According to Klein, the tinmen employ for soldering, a mixture of 1 part of bismuth, 24 parts of tin, and 4 parts of lead. Eight parts of bismuth, 3 of tin and 5 of lead, gave a metal exceedingly like tin in its colour and brightness, but very brittle; in water beginning to boil, it became not only soft, but was completely fused. This imitation, however, may be better accomplished by the mixture of Professor Lichtenberg, which consists of 5 parts of bismuth, 3 of tin and 2 of lead. This metal is very like the former, though not so brittle; but it seemed to melt in hot water even before it came to boil.

VI. *Physical Observations on the East or Baltic Sea. By* F. W. Otto. *From* Abriss einer Naturgeschichte des Meeres. *Berlin* 1792 *and* 1794. 2 *Vols.* 8*vo.*

THE Baltic is a real mediterranean sea, and not a lake, as M. de Buffon endeavours to shew, in his general work on natural history. It is every where surrounded by the land, except where it is connected with the North Sea by three narrow passages, the Sound *, and the large and lesser Belt. At

* The passage through the Sound is now of less importance since the construction of the Holstein canal, which joins the Baltic to the German Ocean. The idea of this junction was conceived under Frederic IV. Duke of Schleswig Holstein, but was not undertaken till the Russian government agreed to co-operate in promoting its success. It was begun in the spring of the year 1777, and was carried on by contractors, who engaged, for a certain yearly sum, to complete a certain portion of it. This canal, the whole length of which, from Kieler-Ford to Rendsburg, is equal to 10,650 poles, of sixteen feet each, proceeds on a level with the Baltic to the first lock at Holtenach, where it rises eight feet six inches. It then proceeds to the second lock at Knop, 745 poles distant from Kieler-Ford, which has a rise of eight feet six inches, and then continues to near

At Pillau and Memel it communicates with two large lakes, the Friſch Haff and Curiſch Haff; which, however, both contain freſh water. The former, according to the account of ſome writers, was occaſioned by a violent wind, which prevailed in the year 1190, and, continuing for more than twelve years, drove up ſo much ſand from the bottom of the ſea that the preſent tongue of land was raiſed; and the gulph, which now forms the Haff, ſeparated from the Baltic. But this opinion is the more to be doubted, as the

near Suenſdorf, where the third lock is ſituated, having a riſe of the ſame height. Here the upper canal begins, and proceeds for the diſtance of 2413 poles, between Schwartenbec and Wittenbec, to the fourth lock at the Upper Eyder near Schinkel. This upper canal, which ſerves as a reſervoir, has an influx of water from the neighbouring lakes ſufficient for the purpoſes of navigation, and is twenty-five feet ſix inches higher than the level of the Baltic. At the fourth lock the canal falls ſeven feet four inches two lines; proceeds 1438 poles in the Eyder to the fifth lock at Nedderholten, where there is alſo a fall of the ſame height; and having continued by Seeſtede to Steinwarp, 2901 poles, little more art is employed, becauſe the Eyder between that place and Rendſburg has almoſt naturally the ſufficient depth and breadth. A ſixth lock is conſtructed at Rendsburg, as the tide flows up there in the Eyder, and makes with the ebb a difference of one foot ſeven inches. The breadth of this canal, at the bottom, is fifty-four feet, and, at the ſurface of the water, ninety feet. It is nine feet deep, and navigable for ſhips of from 150 to 160 tons burthen. The locks, therefore, between the gates, are 100 feet in length and 27 feet in breadth. Along the banks there is a path ten feet broad, and another of twelve feet, for the horſes which are employed to draw the veſſels. The number of veſſels which paſſed through this canal in 1797 amounted to 2105, of which 1393 were laden with merchandize and the reſt in ballaſt. Of the former 41 were Daniſh veſſels bound to foreign ports; and 129 bound from foreign countries to Daniſh or other northern ports; 539 were from the Baltic; 170 from the North Sea; and 78 were bound to or coming from Altona. Among the foreign ſhips were 24 Engliſh, 22 Dutch, 430 from Eaſt Frieſland, 191 from the duchy of Oldenburg, 9 from Jevern, 89 from Popenbourg, 9 from Bremen, 8 from Hamburgh, 6 from Lubec, 44 from Hanoverian ports, 81 from Mecklenburg, 81 from Swediſh Pomerania, 106 from Pruſſia, 11 from Courland and Ruſſia, and 62 from Sweden. EDIT.

Curiſch

Curiſch Haff is of the ſame nature with the Friſch Haff, and yet no one refers its origin to any tempeſtuous wind of the like kind. It is, on the other hand, certain that great changes have taken place in regard to the ſtrait by which the Baltic is connected with this lake. At firſt it was ſituated immediately under the mountain on which the caſtle of Lochſtedt ſtands, and at that time was deep and roomy; but in the year 1311, or according to other accounts 1395, it was entirely choaked up with ſand by an uncommonly violent ſtorm of wind which prevailed in the months of Auguſt and September; and this account is highly probable, as the valley between the Haff and the ſea ſtill ſhews evident traces of the courſe of the water, while the ſand hills, raiſed towards the ſhore, are a further proof of this natural revolution. An opening was conſequently formed oppoſite to the caſtle of Balga, between the villages Faglee and Schineergrube, which was fifteen ells in depth; but which, in a few years, experienced a like fate, being choaked up with ſand. The irruption of the Haff then followed, cloſe to the ſo called Pfundbude or light-houſe, ſituated on a mountain as a guide to ſhips which enter it, and the water was nine fathoms in depth; but, at length, this ſtrait being filled up with ſand, the Haff became united to the ſea at Pillau, in the year 1510. The depth of this opening was at firſt nine fathoms; but it from time to time increaſed, ſo that, at preſent, it can be entered by the largeſt veſſels.

It has long been obſerved that the water in the Baltic is cooler, even in the hotteſt ſummers, than that of other ſeas. The waves alſo never riſe here to ſuch a height as in the North Sea, but they follow each other with greater rapidity. In calm weather, therefore, it breaks with much leſs violence on the coaſt than is the caſe in other ſeas, when the weather is equally ſtill. It has been remarked alſo, that when it breaks on the ſhores of Pruſſia, in particular, there ariſes a ſtronger ſpray and agitation than on any of the neighbouring coaſts of other countries; and this is ſaid to be obſerved,

above

above all, at the time when the firſt ſnow appears, and in the beginning of the ſpring.

In the next place, the water of the Baltic is leſs ſaline than that of other ſeas; the cauſe of which, in all probability, is to be aſcribed to the great number of rivers that fall into it. For this reaſon its gravity is likewiſe leſs, and veſſels cannot ſail in it with the ſame velocity as in the North Sea. For the ſame reaſon alſo it is more liable to corruption. The number of the ſtreams which empty themſelves into this ſea amount, according to Buffon, to forty; and among theſe the Oder, the Weichſel, &c. are the moſt conſiderable. Notwithſtanding the great quantity of water ſupplied by theſe rivers, the Baltic does not ſenſibly increaſe. Its principal efflux, towards the North Sea, is the Sound; though Engliſh navigators have found there, at the depth of four or five fathoms, a ſunken current, running in a contrary direction to that at the ſurface.

The Baltic is not ſubject to a regular ebbing and flowing, as it is ſurrounded by land, and is united with the North Sea only by the Sound and the two Belts: this circumſtance has given occaſion to its being called the inactive ſea (*mare pigrum*). During a long continuance of the weſt wind its natural efflux is prevented, and a great deal of water forced into it from the North Sea; ſo that it then riſes on the coaſts a little above its uſual level. This connection, however, with the German ocean is ſometimes the cauſe that the ebbing and flowing of the latter, though weak, cooperates with the Baltic, ſo that traces of their effects may be ſometimes perceived.

We are aſſured by hiſtory that this ſea has been ſometimes totally frozen during ſevere cold. This was the caſe in the year 1333, at which time people could travel on the ice from Lubec to Pruſſia and Denmark; and on this account huts were erected in different places for the accommodation of travellers. A like phenomenon occurred in 1399 and 1533; and in 1423 people could walk and ride over the

ſea in a ſtraight line from Konigſberg to Lubec. Six years after this ſea was every where covered with ice, in ſuch a manner that journeys of the like kind were undertaken not only from Pruſſia to Holſtein, but alſo from Mecklenburg to Denmark; and this was done likewiſe in 1459. In the ſtrong froſt of the year 1709, the ice extended ſo far on the Baltic, from the Pruſſian coaſt, that people on the higheſt ſteeples on the ſhore could not ſee where it ended; and this no doubt was the caſe during the cold of the year 1740, which exceeded the former ſome degrees.

The depth of the Baltic, in moſt places, never exceeds fifty fathoms. In ſome few places of the Gulph of Bothnia no bottom indeed is to be found; but in others, quite near, the depth is not more than fifty fathoms.

In regard to the decreaſe of water in the Baltic, much has been ſaid for and againſt it; but nothing certain can be determined on this point. Mr. Otto is of opinion that the contradictions, apparent on the firſt view of the ſubject, might be reconciled by attending to the times. The period of the decreaſe of water in this ſea, of which the ſtill exiſting remains of marine productions are incontrovertible proofs, belongs to the moſt remote ages. Since the time when the Baltic was confined within its preſent boundaries, the decreaſe and increaſe of its water are merely apparent; as it may have happened, from various cauſes, that land may have been gained in one quarter and loſt in another. Large rivers, which flow with great rapidity, may, for example, have carried with them into the ſea a great deal of earth and ſand, by which the beds at their mouths may have been raiſed, and the banks extended towards the ſea.

VII. *Examination of the Experiments and new Obſervations of* G. PEARSON, *M.D. on Human Urinary Concretions; and a Compariſon of the Reſults obtained by that Chemiſt with thoſe of* SCHEELE, BERGMAN, *and ſome of the French Chemiſts. By M.* FOURCROY. *From* Annales de Chimie, *Vol. XXVII.*

[Concluded from page 272.]

MY view in commencing my labours on this ſubject was not only to eſtabliſh the facts announced by Scheele, but alſo to purſue much farther the examination of urinary calculi; and to add to it that of all the animal concretions I could procure. This plan will be found partly executed in the details I have publiſhed. 1. On the inteſtinal calculus of the horſe, which I found to be a triple ſalt, formed of 2 parts of phoſphat of magneſia and 1 part of the phoſphat of ammonia. 2. On a renal calculus of the ſame animal, in which I diſcovered 3 parts of the carbonat of lime and 1 part of the phoſphat of lime, without any matter analogous to that in the human calculus. 3. On a calculus of a cat, which gave me 3 parts of the carbonat of lime and 1 part of the phoſphat of lime. 4. On the tartar of the teeth, which I found to be pure phoſphat of lime. 5. On the calculus of the human reins, the nature of which I determined to be perfectly ſimilar to that of the calculus of the bladder. In the examination of ſeveral concretions, different from thoſe of the urinary calculus, I was prior to Dr. Pearſon by more than ten years, and my labours had been publiſhed five years when he communicated his to the Royal Society of London; yet he has not quoted my work, though he has done nothing but repeated and confirmed my experiments.

With regard to the human urinary calculus, I had examined a ſufficient number to be able to trace out the general characters which I thought it neceſſary to add to

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thoſe given by Scheele. Having indeed repreſented it, after this illuſtrious chemiſt, as a ſolid acid cryſtalliſed in laminæ, inſipid, giving a faint red tinge to blue colours, ſoluble in cauſtic alkalies and in the nitric acid, aſſuming with the latter a beautiful red colour, decompoſable by fire, yielding a great deal of the carbonic acid and little oil; the weakeſt of all the acids; containing lime and alkaline phoſphats only by accident. I add to theſe characters the following properties eſtabliſhed by the analyſis of a great number of varieties of theſe concretions: 1. Their ſolution in water reddens turnſole paper. 2. They give the pruſſic acid by diſtillation, and by the action of the nitric acid. 3. The calculus of the human bladder contains but little hydrogen, ſince it gives but little oil; and but little oxygen, ſince it furniſhes but a very ſmall proportion of the pruſſic and carbonic acids.

The experiments which I afterwards deſcribed on the four calculi carefully treated by different agents, may be taken, according to all the other experiments, which I did not think it neceſſary to deſcribe in the ſame manner, as an account of the properties of the urinary calculus conſidered as a genus; and I muſt indeed here obſerve, there are none of them which do not preſent reſults more or leſs ſimilar. Thus: 1. The augmentation of its weight in water, into which the calculus was entirely immerſed. 2. The earthy ſmell of marl which it diffuſes, when diluted as a powder in this liquid. 3. The little alteration it exhibits, and the imputreſcibility it preſerves during more than fifteen days under water at a temperature above 12 degrees (57° Fahr.). 4. Its almoſt perfect ſolubility in 2000 times its weight of water, when repeatedly treated in powder. 5. Its ſolubility in leſs than half that quantity of boiling water; its ſeparation only partial in lamellated cryſtals by cooling—the manner of obtaining it thus pure. 6. Its property of reddening turnſole paper, when after this purification it is rubbed on this paper with a little water. 7. Its ſolution in lime water,

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which by expoſure to the air ſoon depoſits both carbonat of lime and the lithic acid ſeparate from each other, proves that the atmoſpheric carbonic acid decompoſes the calcareous lithiat which was there formed. 8. Its almoſt total ſolubility (except $\frac{1}{12}$) in a ley of cauſtic potaſh, which often diſengages much ammonia. 9. Its precipitation of a golden yellow colour from this alkaline ſolution by the acetous acid, which ſeparates from it the lithic acid in ſmall white brilliant and almoſt pulverulent needles, and which furniſhes more than a half of the cryſtalliſed lithiat of potaſh. 10. This precipitation of the lithic acid from the alkaline ſolution by the acetous acid, given as a good proceſs for procuring this animal acid pure. 11. The ſolubility of the calculus ſometimes entire, ſometimes in powder, in the oxygenated muriatic acid, which at firſt afforded me the hope of a lithontriptic; but which being deſtroyed by other ſucceſſive experiments preſented only a ſingular fact worthy of being further inveſtigated. 12. The action of the fire and diſtillation in a retort upon the calculus of the human bladder, uſing a quantity five times greater than that employed by Dr. Pearſon, and which gave me as products near a quarter of a ſublimated lamellated acid, ſtill analogous to the lithic acid; ſome drops only of water and thick oil, fixed, containing a little pruſſiat of ammonia; $\frac{1}{8}$ concrete carbonat of ammonia, a bulk of more than ſix kilogrammes of water in gas, $\frac{2}{3}$ of which were carbonic acid; a coal weighing a little more than a quarter of the calculus, and which yielded only $\frac{1}{80}$ of its weight of aſhes, without any ſenſible trace of lime.

All theſe facts, which I was obliged to concentrate here in ſome manner in order to render them more ſtriking, ſerved to confirm the firſt reſults of Scheele and Bergmann; and enabled me to add ſeveral obſervations which had eſcaped them. I had concluded with them that the human urinary calculus, whether that of the reins or that of the bladder, contained a matter different from all other animal ſubſtance,

ſtance, not found in any other humours of the human body, nor in any of thoſe of the bodies of the different animals now known; a weak concrete acid, almoſt inſoluble, the principal ſolvents of which were the cauſtic alkalies; that this particular acid very little hydrogenated and oxygenated, but much charged with carbon and azot, was an immediate production of the reins and of the diureſis, or of the formation of urine; that it was ſometimes joined with ſome parts of the phoſphats of lime, of ſoda, and of ammonia, with a colouring animal matter; but that theſe different ſubſtances, foreign to the lithic acid, ſeemed to be only acceſſories, variable in their proportions, which might not have been found there at all, and which did not ſeem to conſtitute the particular eſſence of it.

I may add to this notice of my labours, which are already pretty old, that the experiments I have had occaſion to make for ten years paſt on this animal matter, either for ſome particular purpoſe or in the courſe of my annual lectures, by confirming me more and more in my former ideas, agreeing with thoſe of Scheele and Bergmann, have only taught me that ſome human urinary calculi contain phoſphat of lime, inſoluble in water and in pure alkalies, and the alteration which the lithic acid experiences by the action of the nitric acid when boiled in the latter—an alteration during which there is diſengaged carbonic acid gas, azotic gas, and the pruſſic acid gas; ſo that the calculous matter appears to me really to change its nature during this action of the nitric acid. But all this ought not to change any thing of my opinions in regard to the particular character and acid properties of the peculiar matter of the human urinary calculus.

C. Fourcroy then aſks, whether the labours of Dr. Pearſon have given different reſults, and of ſuch a nature as ſhould induce the French chemiſts to give up their former ideas reſpecting the nature of the peculiar matter of the human urinary calculus; or whether his experiments are ſufficiently

concluſive to induce them to admit its non-acidity, and conſider it as an oxyd? He inſiſts that Dr. Pearſon has not correctly comprehended the labours either of the Swediſh or French chemiſts; that he has improperly believed that they gave the name of lithic acid to the ſublimate of the calculus obtained by fire, whereas they gave it, either to the entire calculus as it comes from the reins or the bladder, when wholly formed of that ſubſtance, or, to this matter obtained by cooling a ſolution of it in water, or by precipitating it from an alkaline ſolution by the acetous acid; and that the pretended oxyd is really Scheele's acid, at firſt called the bezoardic, and ſince, in the new nomenclature, the lithic acid.

Dr. Pearſon gives a larger proportion of inſoluble matter than his predeceſſors, ſtating it at almoſt $\frac{1}{3}$d of the mixed calculi which he employed; but Fourcroy oppoſes to this what Dr. Pearſon ſays in another place, "that he found ſome calculi contain even $\frac{199}{200}$ths of the ſoluble part;" and remarks, that there is no reaſon to doubt that the greater part of human urinary concretions contain a much greater proportion of matter ſoluble in alkalies than that of which he has given the analyſis. The diſengagement of ammonia obſerved by Dr. Pearſon, which accompanies the ſolution of the calculus in pure ſoda, was alſo ſeen by Scheele, and obſerved and carefully deſcribed by Fourcroy.

The ſoapy and unctuous feel, ſays Fourcroy, of the ſpontaneous depoſit formed in the alkaline ſolution of the calculus manifeſtly ariſes from the ſuperabundance of alkali; we perceive here the ſeparation of our lithiat of potaſh from its ſaturated ſolution; and I may remark, that this is what I have pointed out in my reſearches, by ſaying that the lithiat of potaſh eaſily ſeparates in cryſtals from its concentrated and ſaturated ſolution. This ſoapy depoſit of Dr. Pearſon is nevertheleſs ſoluble in water, and precipitated by acids without being re-diſſolved: this is a decompoſition of the lithiat

of potaſh and the ſeparation of the lithic acid. According to the author, this ſolution was not rendered turbid by the carbonic acid. I had made exactly the ſame obſervation.

The precipitate formed in the alkaline ſolution of the calculus by the ſulphuric acid is of principal importance to my inveſtigation, ſince it is this precipitate which forms the real difference between us, and to which our whole attention ſhould be directed. It is really our lithic acid which Dr. Pearſon will not acknowledge as an acid, and in which he thinks he has found a ſufficient number of characters, different from what I had aſſigned to it, to induce him to conſider it as a peculiar animal matter, and to name it the uric or ouric oxyd. Inſipidity, the quality of being inodorous, inſolubility in the mouth, ſolubility in 800 parts of boiling water, the warm ſolution not reddening turnſole paper, cryſtalliſation by cooling, ſolubility in cauſtic ſoda and lime water, inſolubility in alkaline carbonates, non-precipitation of ſoap, but precipitation of alkaline ſulphurets, ſolubility in the nitric acid, and the red pink colour which its nitric ſolution aſſumes when evaporated—theſe are all the reſults of the experiments of Dr. Pearſon, none of which, except the non-coloration of turnſole, differ from what Scheele and myſelf have given; but this difference can have ariſen only from the manner of performing the experiment, or from Dr. Pearſon having made his on ſome matter foreign to the lithic acid. The ſhade of the colour of the nitric ſolution which Scheele announced to be of a beautiful red or blood red, Dr. Pearſon ſays was a pink red; and it is to this tint, which he alludes to ſeveral times in his diſſertation, that he attaches the peculiar character of his uric oxyd.

As to the action of fire either on the human urinary calculus entire or on the precipitate from its alkaline ſolution by the ſulphuric acid, which I have ſhewn to be our pure lithic acid, I have deſcribed with much more care and accuracy than he has in his Memoir, the products of the diſtil-

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lation of this matter which I obtained from a quantity at leaſt ſix times as large as that which he employed.

In what regards the acid and cryſtalline ſublimate obtained in the diſtillation, except the apparatus of a bent-tube deſtined to ſeparate the products, I can ſee nothing new; only here the author gave himſelf needleſs trouble to ſearch for our lithic acid in this manner, ſince, as I have already ſaid, it was not this product of the ſublimation and decompoſition by fire that I diſtinguiſhed by the name of the lithic acid in our nomenclature.

The attempts made by Dr. Pearſon to acidify his ſuppoſed oxyd by the nitric acid and the oxygenated muriatic acid could not poſſibly ſucceed: like thoſe who preceded him, he found that the calculus was changed into carbonic acid and ammonia.

Reſpecting the portions of the calculi not ſoluble in alkali, and which, Dr. Pearſon aſſerts, exhibited to him all the properties of phoſphat of lime, I ſhall obſerve, that if by this fact he ſeems to deviate more from thoſe who went before him, it was only from two calculi, mixed in equal parts, that he obtained ſuch a reſult, and that the very variable difference of the proportion of the ſubſtances ſoluble and inſoluble in the 300 urinary concretions, which he ſays he examined comparatively, can furniſh no oppoſition to the reſult of Scheele in regard to the matter called *lithic acid*.

As to the product of the numerous experiments which Dr. Pearſon tried on more than 300 human urinary calculi, which he compared with each other, I find no other difference between what he announces and what was announced by his predeceſſors, but the variety of the nature which theſe experiments ſhewed to him between theſe concretions—a variety which, however, always ſhews the greateſt proportion in the kind of matter called by him the ouric oxyd. It is in this reſult that Dr. Pearſon deviates moſt from Scheele, who aſſerted that all the calculi of the human bladder reſembled

each other and exhibited no difference. We muſt however add to this fact, which forms one of the new points of the diſſertation of Dr. Pearſon, that of the diſcovery of his ouric oxyd in arthritic concretions. But it may be readily perceived, that this difference between our author and the chemiſts whom he ſeems to combat no way affects the intimate nature of the real calculous ſubſtance, and it is only on the latter that it is of importance to fix the opinions of philoſophers.

The queſtion between us is now reduced to this ſimple point: Ought the properties of the matter which conſtitutes the urinary calculus to induce us to conſider it as a peculiar acid, or only as an animal oxyd?

It is ſufficiently agreed, that every ſubſtance more or leſs ſoluble in water, that turns ſome blue colours red, that, above all, is ſuſceptible of uniting ſtrongly and readily with alkalies, and of forming with them cryſtallifable compounds, in ſuch a manner that their peculiar attractive power for each of theſe ſaliſiable baſes, as well as their affinities, compared to that of other well known acids, may be eſtimated, ought to be ranked among the number of acids. Thus the metals called *tungſten*, *molybdena*, and *chrome*, ſaturated with oxygen by complete combuſtion, have naturally aſſumed a place in the claſs of and next to the moſt powerful and longeſt known acids. It is thus in particular that the matter contained in the gall-nut which blackens iron; that which colours it blue, and which is furniſhed by the decompoſition of animal ſubſtances, have received the character of acid bodies, and the names of the *gallic acid* and *pruſſic acid*. It is, in ſhort, from the ſame conſiderations that our *lithic acid* has been placed in the ſame claſs or referred to the ſame genus. It has not indeed any ſour taſte, and is very little ſoluble in water; but it reddens turnſole paper when it is rubbed on it with a little water, and it combines rapidly with cauſtic alkalies, and brings them to the ſtate of ſalts by ſaturation. If it does not decompoſe ſoap or alkaline carbonates,

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it precipitates the ſulphures; and we muſt not forget that Scheele repreſents it as the weakeſt of all acids. Why then ſhould Dr. Pearſon, without any new facts reſpecting its characters, wiſh to include it among the oxyds? Are the latter even taken in the claſs of ternary or quaternary oxyds, vegetable or animal, ſoluble like it in alkalies; and do they ſaturate them? Is this owing to its little ſolubility? But the ſaccharine oxyd is very ſoluble in water without ſaturating alkalies. Is it not manifeſt, on the contrary, that it wants one of the principal characters of theſe complex oxyds, among which the French pneumatic chemiſtry claſſes a great number of organic compounds? Does it not reject this acidification, ſo eaſy in other oxyds of the ſame kind, that change ſo rapidly into the oxalic or acetous acids by the action of the nitric and oxygenated muriatic acids, as Dr. Pearſon has ſo carefully confirmed? How came he not to perceive, that this difference between it and the other vegetable or animal oxyds required from chemiſts that they ſhould place it in another claſs of bodies; and that, too much oxygenated, no doubt, to paſs to a new ſtate of acidity, being already itſelf as much ſo as the nature of its compoſition would admit, it could not but loſe its combination by the ſtrong action of the oxygenating compounds, and paſs, as was really the caſe, to the laſt term of animal decompoſition, the ſtate of a double binary compound; the carbonic acid and ammonia? I am even aſtoniſhed that Dr. Pearſon, in analyſing this body with that ſpirit of inveſtigation which he employed, was not more inclined than other chemiſts to preſerve the acid character to this compound, and that he was not averſe to compare it with oxyds, ſince he found in it ſo many characters oppoſite to thoſe of the known oxyds. If we indeed take from the compound oxyds contained in organic bodies that character, ſo ſtriking, of becoming acid by a greater proportion of oxygen, we ſhall at once efface every thing clear and preciſe which this denomination preſents to the mind, and ſubſtitute at once darkneſs for light.

All that Dr. Pearſon has done gives more ſtrength ſtill to the well founded opinion which has cauſed this body to be claſſed among the acids. It differs particularly from the oxyds by its unalterability, its imputreſcibility, its ſolubility in alkalies, its reſiſtance to acidification; and it would be deranging to no purpoſe the reſults and opinions of modern chemiſtry to place it in that claſs of compounds with which it in reality has no relation. I ſhall then continue with all the French chemiſts to conſider human urinary calculi as containing, moſt frequently, and, ſometimes, altogether, a weak acid, the characters of which were well determined before Dr. Pearſon, and even confirmed by his reſearches.

I ſhall add one important obſervation, that of all the acids it is that which cryſtalliſes the ſooneſt, and whoſe moleculæ perhaps are moſt altered; which aſſumes with the greateſt eaſe the concrete and ſparry form, under which, as it is ſeen in regular calculi and in compact ſtrata, it almoſt eludes the action of the moſt powerful re-agents and ſolvents; and it is to this laſt property, the moſt terrible and afflicting for humanity, that what is called the ſtone in the bladder muſt without doubt reſiſt the action of all the known ſolvents, and has hitherto given no hopes of the diſcovery of a lithontriptic.

In the preſent ſtate of chemiſtry the particular matter of human urinary calculi appears only as a combination, where abundance of carbon and azot united to a ſmall quantity of hydrogen are inclined to a ſlight acidification by the fixed oxygen which they contain. The proportion even of theſe primitive materials has not yet been determined. After having examined the labours of Dr. Pearſon, I muſt agree with him, that the name *lithic acid* given to this animal concretion, and which has been borrowed from the medical term lithiaſis, deſerves that reproach of impropriety which he beſtows on it. I am of opinion that we might adopt with advantage that of *ouric acid*, ſince this body has not yet been found but in the urine of man; and that even if future expe-

riments ſhould confirm the diſcovery of Dr. Pearſon, who ſays he found the ſame acid in arthritic concretions, this denomination would ſtill be very proper, either becauſe the conſtant ſeat of this acid is the human urine, or becauſe it is always more abundant there than in articulations attacked by the gout; or, in the laſt place, becauſe it appears, in ſome meaſure, in fits of that diſeaſe, to be driven and carried from its uſual channel towards the extremities of the bones, the articular membranes of which it irritates.

VIII. *On the Invention of the Euphon, and other acouſtic Diſcoveries of* C. F. F. Chladni, *Doctor of Philoſophy and Law, Fellow of the Royal Society of Gottingen, correſponding Member of the Imperial Academy of Sciences at Peterſburgh, &c.*

MANY perſons who intereſted themſelves in favour of Dr. Chladni and his inventions having expreſſed a wiſh of becoming acquainted with the hiſtory of them, the Doctor drew up a ſhort paper on that ſubject, which he tranſmitted to one of the German Journals*, and from which we have extracted the following particulars. The author firſt obſerves, that the invention of the euphon and all his other acouſtic diſcoveries were not the effects of chance, but the fruit of long and continued reflection. The rather confined education which he received, both in his father's houſe and at the ſchool of Grimma, did not produce on him the uſual but a contrary effect, as it inclined him very early to think and to act for himſelf. At the age of only ſix or ſeven, when he ought to have been otherwiſe employed, he would ſit whole hours ſtudying maps and books of geography, and wiſh, at the ſame time, that he might have an opportunity of travelling over all the countries which he ſaw repreſented before him. His father, firſt profeſſor of law at Wittenberg,

* Magazin für das neueſte aus der Phyſik, vol. ix. part 4, p. 100.

who, according to the then uſual cuſtom, had converted the original Hungarian family name into that of Chladenius, was deſirous that the author, his only ſon, ſhould apply himſelf to the law, though it was contrary to his inclination. Dr. Chladni, therefore, ſtudied law firſt at Wittenberg, and then at Leipſic, and, after the uſual forms, took his degree of doctor. On his father's death he quitted this line, for which he had no attachment, and, devoting himſelf entirely to the ſtudy of nature, delivered lectures on phyſical and mathematical ſubjects to qualify himſelf for a profeſſorſhip which he had ſome hopes of obtaining. In his 19th year he began to learn to play the harpſichord; and he afterwards read a great many of the principal works on the theory of muſic, by which he found that the phyſico-mathematical part of that ſcience was far more defective than other branches of natural philoſophy. Being therefore poſſeſſed with an idea that his time could not be better employed than in endeavouring to make diſcoveries in this department, he accordingly tried various experiments on the vibrations of ſtrings and the different kinds of vibration in cylindric pieces of wood, firſt diſcovered, through calculation, by the elder Euler; and found that, though a great deal had been ſaid on the nature of theſe elaſtic bodies, yet the manner of vibration and the proportion of tones in other elaſtic bodies, which do not proceed, as in the former, in ſtraight lines, but depend on the vibration of whole ſurfaces, were totally unknown, and that the little which had been written on that ſubject, by ſome authors, did not correſpond with nature. He had already long remarked, that every plate of glaſs or metal emitted various tones according as it was held and ſtruck in different places; and he was deſirous to diſcover the cauſe of this difference, which no one had ever examined. He fixed in a vice the axle of a braſs plate which belonged to a poliſhing machine, and found, that by drawing the bow of a violin over it, he produced very different tones, which were ſtronger and of longer duration than thoſe obtained merely

merely by ſtriking it. The obſervation, that not only ſtrings but alſo other elaſtic bodies may be made to produce ſounds by drawing a violin bow over them, Dr. Chladni does not give as a diſcovery of his own; as the ſo called iron violin has been long known, and as he had read of an inſtrument, conſtructed in Italy *, where glaſs or metal bells were made to ſound by means of two or more violin bows drawn over them. But the idea of employing this inſtrument to examine vibrating tones was firſt entertained by himſelf. Having accurately remarked the tones produced by the above-mentioned metal plate, he found that they gave a progreſſion which correſponded with the ſquares of 2, 3, 4, &c. Not long before he had read, in the Tranſactions of the Royal Society of Gottingen, the obſervations of Mr. Lichtenberg on the phenomena produced by ſtrewing pounded reſin over a glaſs plate or cake of reſin, and he repeated many of his experiments. This led him to the idea that, perhaps, the various vibratory movements of ſuch a plate would be diſcovered by a diverſity of phenomena, if he ſtrewed over it ſand or any thing of the like kind. By this experiment there was produced a ſtar-formed figure; and the author, having continued his reſearches, publiſhed the reſult of them in a work entitled, Diſcoveries reſpecting the Theory of Sound, printed at Leipſic in 1787 †. It contains a multitude of new obſervations on the vibrations of different bodies, the proportions of the tones of which had been incorrectly given by others; on the vibrations which are produced by vibration and rotation conjointly; on the longitudinal vibrations of a ſtring, firſt announced by the author; and upon various other circumſtances relating to acouſtics. The author addreſſed this work to the Academy of Sciences at Peterſburgh, with a view, perhaps, that it might give occaſion to analytical inveſtigations on the ſame ſubject. J. Ber-

* In all probability the harmonica of the Abbé Mazzuchi.

† Entdeckungen über die Theorie des Klanges, Leipſic, 1787. 4to.

noulli,

noulli, who, next to the elder Euler, had made conſiderable additions to the ſcience of acouſtics, and who unfortunately loſt his life ſoon after, while bathing in the Neva, endeavoured, in conſequence of the above work, to determine by calculation the vibrations of a ſquare plate; but the reſult did not agree with experience, and Dr. Chladni does not think it poſſible to advance one ſtep in this theory by analyſis alone.

While Dr. Chladni was employed in theſe inveſtigations, his ſituation was exceedingly unpleaſant. He, however, did not ſuffer his courage to be depreſſed, but exerted his abilities the more, in order that he might procure for himſelf better means of ſubſiſtence. As he had examined the nature of ſo many ſonorous bodies, he reſolved to invent a new muſical inſtrument; and he began to conſider whether it might not be poſſible by rubbing glaſs tubes in a ſtraight line, with the wet fingers, to produce ſounds in the ſame manner as is done in the harmonica by rubbing them circularly. That glaſs tubes, like thoſe in his euphon, would not merely by ſuch rubbing emit any tones, he had long known by theory and experience; and he therefore applied himſelf to the ſolution of the difficult queſtion, in what manner the inſtrument ought to be conſtructed to anſwer the intended purpoſe. After various fruitleſs attempts for a year and a half, during which his imagination was ſo full of the idea, that ſometimes in his dreams he thought he ſaw the inſtrument and heard its tones, that is, like thoſe of the harmonica, but with more diſtinctneſs and leſs confuſion, he at length, in a ſtate between ſleeping and waking, obtained a ſolution of the problem which had given ſo much employment to his thoughts. On the ſecond of June 1789, being tired with walking, he ſat down on a chair, about nine in the evening, to enjoy a ſhort ſlumber; but ſcarcely had he cloſed his eyes when the image of an inſtrument, ſuch as he wiſhed for, ſeemed to preſent itſelf before him, and terrified him ſo much that he awoke as if he had been ſtruck by an electric ſhock. He imme-

immediately ſtarted up in a kind of enthuſiaſm; and made a ſeries of experiments which convinced him that what he had ſeen was perfectly right, and that he had it now in his power to carry his deſign into execution. He made his experiments and conſtructed his firſt inſtrument in ſo private a manner, that no perſon knew any thing of them. On the 8th of March 1790 his firſt inſtrument of this kind was completed; and in a few days he was able to play on it ſome eaſy pieces of muſic. It was now neceſſary to give to this inſtrument, as it was entirely new, a new name, and that of *euphon*, which ſignifies an inſtrument that has a pleaſant ſound, appeared to him the moſt proper.

The firſt inſtrument of this kind made by Dr. Chladni was in ſound, and particularly in the higher tones, as good as thoſe which he made afterwards; but the conſtruction was deficient in ſtrength, ſo that every week ſome hours were neceſſary to keep it in proper repair; and it was impoſſible to convey it the diſtance of a mile without almoſt totally deſtroying it. Dr. Chladni alſo for want of better tubes employed thoſe uſed for thermometers, and marked the whole and half tones by a coating of ſealing-wax on the under ſide; but as the wax, owing to the moiſture and vibration, often cracked and flew off, it was attended with danger to the eyes. It was, therefore, extremely difficult to give to the conſtruction of the inſtrument ſufficient ſtrength; but this the inventor at length accompliſhed, ſo that his new euphon cannot be injured or put out of tune either by playing or by carriage. The third inſtrument conſtructed by Dr. Chladni was ſomewhat different from the firſt and the ſecond; as the fore part, which in the two former roſe upwards with an oblique angle, ſtood at right angles, ſo that it could be tranſported with eaſe in a particular carriage made for that purpoſe. Inſtead of the thermometer tubes uſed in the firſt, the doctor now employs tubes of different colours. In the ſecond inſtrument thoſe for the whole tones were of dark green glaſs; but he uſed for the half

tones,

tones, in both, a milk white kind of glaſs. In a word, the euphon has ſome reſemblance to a ſmall writing-deſk. When opened, the above-mentioned glaſs tubes, of the thickneſs of the barrel of a quill and about ſixteen inches long, are ſeen in a horizontal poſition. They are wetted with water, by means of a ſponge, and ſtroked with the wet fingers in the direction of their length, ſo that the increaſe of the tone depends merely on the ſtronger or weaker preſſure, and the ſlower or quicker movement of the fingers. The number of tubes at preſent is forty-two. In the back part there is a perpendicular ſounding-board divided in the middle, through which the tubes paſs. It appears therefore that the euphon ought not to be conſidered as an altered or improved harmonica, but as a totally new and different inſtrument. In regard to ſweetneſs of ſound, it approaches very near to the harmonica; but it has ſeveral advantages which no unprejudiced perſon, who examines both inſtruments, will deny.

In the firſt place it is ſimpler, both in regard to its conſtruction and the movement neceſſary to produce the ſound, as neither turning nor ſtamping is required, but merely the movement of the finger.

2. It produces its ſound ſpeedier; ſo that as ſoon as it is touched you may have the tone as full as the inſtrument is capable of giving it; whereas, in the harmonica, the tones, particularly the lower ones, muſt be made to increaſe gradually.

3. It has more diſtinctneſs in quick paſſages, becauſe the tones do not reſound ſo long as in the harmonica, where the ſound of one low tone is often heard when you wiſh only to hear the following tone.

4. The uniſon is purer than is generally the caſe in the harmonica, where it is difficult to have perfect glaſſes, which in every part give like tones with mathematical exactneſs. It is however as difficult to be tuned as the harmonica.

5. It

5. It does not affect the nerves of the performer; for a perſon ſcarcely feels a weak agitation in the fingers; whereas in the harmonica, particularly in concords of the lower notes, the agitation extends to the arms and even through the whole body of the performer.

6. The expence of this inſtrument will be much leſs in future than that of the harmonica.

7. When one of the tubes breaks, or any other part is deranged, it can be ſoon repaired, and at very little expence; whereas, when one of the glaſſes of the harmonica breaks, it requires much time and is very difficult to procure another capable of giving the ſame tone as the former, and which will correſpond ſufficiently with the ſeries of the reſt.

Though Dr. Chladni has been much occupied with the conſtruction and improvement of the euphon, he has, however, ſtill continued his reſearches reſpecting the theory of ſound, and collected materials for a continuation of his eſſay before mentioned, which contain abundance of valuable matter. In one paper, which may be found in the Berlin Muſical Journal for Auguſt 1792, he has given ſome very accurate obſervations reſpecting the longitudinal vibrations of a ſtring, firſt announced by himſelf, which depend upon laws of nature totally different from thoſe of the uſual vibrations. Dr. Chladni tranſmitted alſo, a few years ago, to the Friendly Society of the Searchers into Nature at Berlin, in order to fulfill his duty as a member, two eſſays, one of which contained obſervations on the tones produced in a tube by burning inflammable air; from which it appears that theſe tones, firſt remarked by Mr. De Luc, are nothing elſe than pipe tones, as the ſonorous body is not the tube but the column of air contained in it. The other eſſay was entitled Collections towards promoting a better Explanation of the Doctrine of Sound. The author here ſhews how this part of the ſcience of nature, not yet ſufficiently elucidated by any one writer, ought to be treated, according to his opinion,

opinion, ſo as to approach neareſt to the propoſed end; and he gives a ſhort ſketch of his method, which contains alſo ſome new obſervations *.

IX. *Account of a ſimple Method of repreſenting the different Cryſtalline Forms by very ſhort Signs, expreſſing the Laws of Decrement to which their Structure is ſubjected.* By C. Hauy. *From* Journal des Mines, *An. IV. No.* 23.

THE different cryſtals belonging to each mineral ſubſtance are connected with one identical primitive form, which, in its turn, ſerves to connect them in common. An accurate knowledge of theſe mutual relations depends upon that of the laws to which their ſtructure is ſubjected, and of which the effect is to determine the number and aſſortment of the planes arranged around the primitive form, in order to produce the ſecondary forms. By a neceſſary conſequence the naturaliſt, who is familiar with the progreſs of theſe laws, needs often only to keep in his eye the primitive form, and an account of the decrements which its angles or edges undergo, to repreſent to himſelf the polyedron thence reſulting, and to ſee, in ſome meaſure, in idea, the metamorphoſis of the nucleus from which this polyedron originates.

Theſe conſiderations induced me to conceive the idea of converting into very conciſe language, analogous to that of algebraic analyſis, a definition of the different laws by which ſecondary cryſtals are determined; and thus to compoſe a kind of formulæ repreſentative of theſe cryſtals. To accompliſh this, it will be ſufficient to diſtinguiſh by letters the angles and edges of the primitive form, and to accompany theſe letters with figures pointing out the decrement which

* Dr. Chladni is the author alſo of a Diſſertation on the Maſs of Iron found by Profeſſor Pallas in Siberia, and of ſome papers on fire-balls. See Phil. Mag. vol. ii. p. 1, 225, and 337.

ſuch

ſuch an angle or edge undergoes, and of which the reſult is this or that ſecondary form. I have endeavoured to ſubject the arrangement of the letters to a regular progreſs in relation with the order of the alphabet; ſo that this arrangement might appear as if occurring naturally of itſelf.

1. Let us ſuppoſe that fig. 1. repreſents an oblique angled parallelopipedon, the faces of which have angles different in meaſure, and which are the primitive form of a particular kind of mineral ſuch as feld ſpar *. Having adopted the vowels to denote, in general, the ſolid angles, place the four firſt A E I O at the four angles of the upper baſis, following the order of the alphabet, and at the ſame time the common mode of writing, which is to begin at the top and to proceed from right to left. (See *fig.* 2. where the arrangement of the letters in lines is rendered ſenſible to the eye.)

2. Having adopted the conſonants to denote the edges, in general, place, according to the ſame rule, the firſt ſix B C D F G H on the middle of the ſides of the upper baſe, (*fig.* 1.) and on the two longitudinal edges of the lateral face, which firſt preſents itſelf from left to right.

3. Laſtly, place on the middle of the upper baſe and the two lateral faces, ſituated in front, the three letters P M T, which are the firſt of the ſyllables that form the word primitive.

4. Each of the four ſolid angles or of the ſix edges, denoted by letters, is ſuſceptible, in the preſent caſe, on account of the irregular form of the parallelopipedon, of undergoing peculiar laws of decrement; but as theſe laws act with the greateſt poſſible ſymmetry, at leaſt in common, every thing that takes place on one of the angles or edges, pointed out, is repeated on the angle or edge diametrically oppoſite among thoſe which remained unoccupied; ſo that the latter is ſuppoſed to perform the ſame function as the

* The parallelopipedon is ſuppoſed to be repreſented in ſuch a manner that the angle B A C, fartheſt from the obſerver, is one of the angles of the baſe.

former.

former. For example, A p (*fig.* 3.), being the ſame form as fig. 1. the decrements which the angle A undergoes, occaſion ſimilar ones on the angle p (*fig.* 3.). The caſe is the ſame with the edge A r, in regard to the edge O p; of I u in regard to E s, &c. After this, nothing is neceſſary but to denote the number of ſolid angles or edges which undergo decrements really diſtinct, becauſe the latter actually contain thoſe which take place on the analogous angles or edges.

5. It will, however, be ſometimes neceſſary to point out theſe laſt angles or edges; and in that caſe we may employ the ſmall letters which bear the ſame names as the large letters employed in figure 1; that is to ſay, that p (*fig.* 3.) be denoted by a; $s p$ by c; $p u$ by b, &c. But it will not be requiſite to mark theſe ſmall letters on the figure. It will be ſufficient to introduce them in the ſign of the cryſtal; becauſe they may be eaſily referred, in idea, each to its proper place.

6. To denote the effects of decrements by one, two, three or more ranges, in breadth, the figures 1, 2, 3, 4, &c. may be employed, as ſhall be explained hereafter; and to denote the effects of decrements by two, three, &c. ranges in height, the fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. may be uſed.

7. The three letters P M T will ſerve to denote either the form of the nucleus, without any modification, when they compoſe alone the ſign of the cryſtal, or the faces which would be parallel to thoſe of the nucleus in caſe the decrements did not extend to their limits; and theſe letters will then be combined in the ſign of the cryſtal with thoſe which have relation to the angles or the edges on which the decrements act.

8. The principal or moſt ſimple decrement, in regard to any ſolid angle whatever, ſuch as O, may take place either on the baſe P or on the plane T, which is to the right of the obſerver, or on the plane M ſituated to the left. But it muſt be remarked that the obſerver is ſuppoſed to move

round the cryſtal till he is oppoſite the angle on which the decrements he is conſidering take place; or, what amounts to the ſame thing, that he is ſuppoſed to turn the cryſtal until the angle in queſtion is found oppoſite to him; and it is in regard to this poſition that ſuch a decrement is ſaid to take place towards his right or towards his left. For example, if the angle A be under conſideration, we muſt conceive that the obſerver, who at firſt was oppoſite to the point O, has placed himſelf oppoſite to A; then, by ſtill ſuppoſing figure 3 to repreſent the ſame ſolid as figure 1, the decrements to the right will be thoſe which take place on the plane A E *s r*, parallel to the diagonal drawn from E to *r*; and the decrements to the left will take place on the plane A I *u r*, parallel to the diagonal which goes from I to *r*. We ſhall ſee hereafter the advantage of viewing in this manner, in regard to the uniformity of method.

9. To denote the firſt of the three decrements of which I have ſpoken, or that which takes place on the baſe P, the indicating figure muſt be placed above the letter. To denote the ſecond, or that which takes place towards the right, the figure muſt have the uſual place of an exponent on the right, and towards the top of the letter; and the third, or that which takes place towards the left, muſt be denoted by placing the figure towards the left, and, in the like manner, towards the top of the letter.

Thus $\overset{2}{O}$ will expreſs the effect of a decrement by two ranges in breadth, parallel to the diagonal of the plane P, which paſſes through the angle E (*fig.* 1); O^3 the effect of a decrement by three ranges, parallel to the diagonal of the face T, which paſſes through the angle I; and ^4O the effect of a decrement by four ranges parallel to the diagonal of the face M, which paſſes through the angle E.

10. In regard to the decrements on the edges, thoſe which take place towards the contour B C F D of the baſe may be expreſſed by a figure placed above or below the let-

ter, according as their effect operates upwards or downwards, departing from the edge to which they are referred; and thoſe that regard the longitudinal edges G, H, may be denoted by an exponent placed either in the uſual manner or on the left of the letter, according as they take place either to the right or the left.

Thus D^{2} will expreſs a decrement by two ranges proceeding from D towards C; C^{3} a decrement by three ranges proceeding from C to D; D, a decrement by two ranges deſcending on the face M; ${}^{3}H^{2}$ a decrement by three ranges proceeding from H towards G, &c. To determine the direction of the decrements to the right or left of any ridge, we muſt proceed as in regard to the decrements which affect the angles (8). For example, the decrements to the left of the edge G will be thoſe which take place in proceeding from E *s* to A *r* (*fig.* 3).

12. In the caſe in which we ſhould be obliged to denote, by means of a ſmall letter, ſuch as *d*, a decrement on the edge, oppoſite to that which bears the large letter D, we ought to conſider the cryſtal as turned upſide down: Thus d^{2} would expreſs a decrement by two ranges aſcending on the inferior baſe *p*; as D^{2} expreſſes one which aſcends on the upper baſe P. For the ſame reaſon *c* would expreſs a decrement by three ranges proceeding from sp^{3} (*fig.* 3.) towards EO.

13. If the ſame angle or the ſame edge undergoes ſeveral ſucceſſive decrements, on the ſame ſide, or ſeveral decrements, in regard to different ſides, we may be ſatisfied with writing one letter accompanied with different figures indicating the decrements.

Thus D^{2}_{3} (*fig.* 1.) will denote two decrements on the edge D; one by two ranges aſcending above the baſe P, and the other by three ranges deſcending on the plane M. ${}^{2}H^{2}$ will denote two decrements by two ranges on both ſides of the edge H, &c.

14. If there are mixed decrements, they may be denoted, according to the ſame principles, by employing the fractions $\frac{1}{2}$ $\frac{2}{3}$ $\frac{3}{4}$, &c. the numerators of which refer to decrement in breadth, and the denominators to decrement in height.

15. It now remains to diſcover a method of repreſenting the intermediary decrements. An example will convey an idea of that which I have adopted. Let AEOI (*fig.* 4.) be the ſame face as fig. 1, and let us ſuppoſe a decrement by a range of double moleculæ, according to lines parallel to $x\,y$, ſo that O y may meaſure lines equal to two ſides of a molecula, and O x lines merely equal to one ſide. This decrement, therefore, may be denoted thus $(\overset{1}{O}\ D^1\ F^2)$: firſt the parentheſis ſhews that the decrement is intermediary; O ſhews that it takes place by one range on the angle denoted by the ſame letter, and that it refers to the baſe; D^1 F^2 indicate, that for one edge of molecula ſubtracted along the ſide D, there are two edges ſubtracted along the ſide F.

16. It is uſeful to have a language to expreſs theſe different ſigns in ſuch a manner that they may be eaſily written when dictated. The ſigns O^2, ^3O might be enounced by ſaying, O two at the right, O three at the left. To enounce $\overset{2}{O}$, $\underset{4}{O}$ we might ſay, O under two, O above four: and laſtly, the ſign $(\overset{1}{O}\ D^1\ F^2)$ might be thus enounced: In a parentheſis O under one, D one, F two.

17. Let us now give an example of the combination of theſe different ſigns in the expreſſion of a compound cryſtalline form. But it will firſt be neceſſary to determine the order according to which the letters that concur to form one expreſſion ought to be arranged. Should we admit the alphabetic order, the reſult would be a ſort of confuſion in the view preſented by the formula. It appears therefore more natural to adopt that order which would direct the obſerver in the deſcription of the ſame cryſtal; that is to ſay, to begin by the priſm or the mean part; to indicate its different faces

as they preſent themſelves to the eye, and then to proceed to the faces of the ſummit, or of the pyramid. This will be illuſtrated by the different examples we ſhall mention in the courſe of this article.

Let *z v* (*fig.* 5.) be the variety of feld-ſpar, named *ſimilaire*, the primitive form of which is repreſented fig. 1. In this variety the plane *n q x r* (*fig.* 5.) reſults from a decrement by two ranges on the edge G (*fig.* 1.), proceeding towards H; the plane *r k s p v x* (*fig.* 5.) is parallel to the plane M (*fig.* 1.), which is concealed only in part by the effect of the decrement. The plane *s u m p* is parallel to the plane T; the pentagon *k z y u s* ariſes from a decrement by two ranges on the angle I, parallel to the diagonal which proceeds from A to O; and, in the laſt place, as this decrement does not reach its limits, the ſummit bears a ſecond pentagon *z l n r k* parallel to the baſe P. All this deſcription may be thus expreſſed by five letters, G^2 M T $\overset{2}{I}$ P.

18. Let us next proceed to parallelopipedons of a more regular form; and let us firſt conſider the caſe in which they differ from the rhomboid. We ſhall ſuppoſe that the parallelopipedon is the ſame as that of fig. 1, the form of which has varied, ſo as to become more ſymmetrical. In conſequence of this variation, certain ſolid or ſalient angles, which were different in the firſt parallelopipedon, have become equal: whatever takes place in the one is repeated in the other; and conſequently they muſt be marked with the ſame letter. Thus, in algebra, certain ſolutions are ſimplified in the particular caſes in which a quantity at firſt ſuppoſed to be different from another becomes equal to it.

19. Let us conceive, for example, that the primitive form is a right priſm, the baſe of which is the oblique-angled parallelograms (*fig.* 6.), and we ſhall have O = A, I = E, &c. We muſt then ſubſtitute on both ſides the ſecond letter for the firſt, as ſeen in the figure. By continuing to run over the different modifications of the parallelopipedon, we ſhall ſee them paſs through different degrees of ſimplicity, ana-

logous

logous to thoſe of the forms themſelves, and we ſhall have ſucceſſively:

20. For the oblique priſm having rhombuſes for its baſes the expreſſion repreſented figure 7;

21. For the right priſm with rectangular baſes that ſeen figure 8;

22. For the right priſm having rhombuſes for its baſes that of figure 9;

23. For the right priſm with ſquare baſes that of figure 10,

24. And, in the laſt place, for the cube that of figure 11. Here the baſe only is denoted by letters, becauſe what takes place in regard to this baſe may be applied to any one of the other faces.

25. For all theſe different primitive forms a method of figures, analogous to that which I have adopted for the oblique-angled parallelopipedon of figure 1, may be followed; but the letters of the ſame name, figured in the ſame manner, need not be repeated.

One example will ſerve to give an idea of this method. Let *ar* (*fig.* 12.) be the moſt common variety of the cymophane, the nucleus of which is a rectangular parallelopipedon, ſuch as that ſeen fig. 8. The ſign of the ſecondary cryſtal will be, $MT^{2}G\,G^{2}\,\overset{1}{B}\,A^{\frac{3}{2}}\,{}^{\frac{3}{2}}A$, in which M correſponds to *gohlnr*, T to *bets*, ^{2}G to *fgnm*, G^{2} to *hetl*, $\overset{1}{B}$ to *dacf*, or *bace*, $A^{\frac{3}{2}}$ to *cfgo*, and $^{\frac{3}{2}}A$ to *cebo*.

To illuſtrate better the ſteps which have conducted to this expreſſion, let us point out, for a moment, all the angles and all the edges by as many particular letters, as if the parallelopipedon were oblique-angled. (*See fig.* 13.)

The ſign then will become $MT\,{}^{2}G\,H^{2}\,\overset{1}{B}\,\overset{1}{F}\,E^{\frac{3}{2}}\,{}^{\frac{3}{2}}O$; but by comparing figure 13 with figure 8, we ſee that H = G, F = B, O = A; by ſubſtituting then in the place of the firſt letters their values, we ſhall have $MT\,{}^{2}G\,G^{2}\,\overset{1}{B}\,\overset{1}{B}\,A^{\frac{3}{2}}\,{}^{\frac{3}{2}}A$, which amounts to the expreſſion above ſhewn, by ſuppreſſing the uſeleſs repetition of $\overset{1}{B}$.

26. It reſults from the preceding, that we muſt avoid con-

founding, for example, $^{2}G\,G^{2}$ with $G\ G$. The firſt ſign denotes decrements which take place on the faces *t* T (*fig.* 8.), proceeding from the edges G towards thoſe which correſpond with them behind the parallelopipedon; and the ſecond denotes decrements which take place on the face M proceeding to meet each other. If the two decrements took place ſimultaneouſly, their repreſentative ſign would be $^{2}G^{2}$.

In the preceding ſigns, each letter, ſuch as G or G^{2}, cannot be applied but to one edge ſituated as that letter itſelf to the right or left; but $^{2}G^{2}$ may be applied indifferently to both edges: for this reaſon it is needleſs to repeat that letter.

27. Let us give a new example, taken from the *topaze diſtique*, called commonly the Saxon topaz (*fig.* 15.). If we ſuppoſe figure 9 to repreſent the primitive form, which is a right priſm, with rhombuſes for its baſes, we ſhall have as the ſign of the variety in queſtion $^{3}G^{3}\,M\,\overset{2.3}{B}\,\overset{1.2}{E}\,P$, which is explained in the following manner: 1. The planes ſimilar to *o t z q r* (*fig.* 14.) ariſe from a decrement by three ranges on each ſide of the edges G (*fig.* 9); 2d, the planes *t y* A *z*, *s y* A *p*, are parallel to the planes M, and thus the preceding decrement has not attained to its limits; 3d, the facets *h k y t v*, *n k y s b*, and *g h k i*, *b u k i*, ariſe from two ſucceſſive decrements on the edges B, one by two ranges, and the other by three; 4th, the facets *a c g h v x m*, and thoſe which correſpond to them, on the other ſide, ariſe from two ſucceſſive decrements on the angles E, one by one range, and the other by two ranges; 5th, in the laſt place, the terminating face *c d f l i g* correſponds to the baſe P of the primitive form.

28. It may be readily concluded from the ſame principles, that the dodecaedron with rhombuſes for its planes, originating from the cube (*fig.* 8.), is expreſſed by the ſingle letter B; that the octaedron originating from the ſame nucleus, has for ſign A, &c.

29. The rhomboid, ſuppoſing it placed under the moſt natural aſpect, that is to ſay, in ſuch a manner that the two ſolid angles compoſed of three planes with equal angles be upon

upon one and the ſame vertical axis, has properly no baſe, but only two ſummits, which are the extremities of the axis. Theſe angles and edges may be denoted as ſeen figure 15. The letter *e* makes known that the angle which bears it is ſimilar to that marked with the ſame large letter; ſo that if all the lateral angles had their indications expreſſed, the three neareſt the upper ſummit would bear the letter E, and the three next to the lower ſummit, and which are viſibly oppoſite to the firſt, would have for their indicative letter *e*.

As the rhomboid has its ſix faces equal and ſimilar, it is only neceſſary to conſider the decrements in regard to one of the faces, ſuch as that which bears the letter P, becauſe all the reſt are only repetitions of the latter. This being laid down, 1ſt, the decrements which depart from the upper angle A, or from the upper edge B, will have their indicating figure placed below the letter A or B; 2d, thoſe which depart from the lateral angles E will be denoted by the ſame letter written twice, one on the right, and the other on the left; 3d, in regard to thoſe which depart from the lower angle *e*, or from the lower edge D, the figure deſtined to expreſs them will be placed above the letter *e* or D.

Let us ſuppoſe, for example, that figure 16 repreſents analogical carbonat of lime, in which the vertical faces *e c p g*, *o g r z*, &c. reſult from a decrement by two ranges on the angles *e* (*fig.* 15.); the oblique faces *m d c e*, *h e g o*, &c. from a decrement by two ranges on the edges D, and the terminating faces *i m e h*, *i f t h*, from a decrement by a range on the edges B, we ſhall have the following ſign $\overset{2}{e}\,\overset{2}{D}\,\overset{1}{B}$.

30. The other primitive forms, after what I have ſaid in regard to the parallelopipedon, will be attended with no difficulty. I ſhall run them over in ſucceſſion. Figure 17 repreſents the expreſſion of the octaedron with ſcalene triangles; figure 18 that of the octaedron with iſoſceles triangles, and figure 19 that of the regular octaedron. To place the figures, which accompany the letters, we ſhall conform to what has been ſaid in regard to the rhomboid. Thus (*fig.* 18.)

we muſt put the figure below for the decrements which proceed from A to B; above for thoſe that depart from D; and at the ſide for thoſe which depart from E.

If we wiſhed to denote the reſult of a decrement by one range on all the angles of the regular octaedron (*fig.* 19.), we ſhould write $A\,{}^{1}A^{1}$; and to indicate the reſult of a decrement by a range on all the edges we ſhould write $B\,\overset{1}{B}$. The firſt of theſe decrements produces a cube, and the ſecond a dodecaedron with rhombuſes for its planes.

31. In ſome kinds of cryſtals, as thoſe of the nitrat of potaſh, the octaedron, the ſurface of which is compoſed of eight iſoſceles triangles, ſimilar four to four, muſt be ſituated as repreſented figure 20, in order that the ſecondary cryſtals may be in the moſt natural poſition; that is to ſay, that the edges at the junction of the two pyramids compoſing the octaedron may be, one in a vertical direction as F, and the reſt in a horizontal direction as B. By comparing fig. 20 with fig. 21, where the letters have been placed as if all the angles and all the edges had peculiar functions, we ſhall readily conceive the diſtribution adopted figure 20, and brought back to the ſymmetry of the real primitive figure; for, in the preſent caſe, we have E = A, D = C, G = F. The indicating figure muſt be placed below the letter for decrements departing from B; and on one ſide or below it for thoſe departing from A, according as their effect ſhall be directed towards B or F.

32. The tetraedron being always regular, when it becomes the primitive form, its expreſſion will be repreſented figure 22. To indicate, for example, a decrement by three ranges on all the edges we muſt put B and $\overset{3}{B}$; and to denote one by two ranges, on all the angles, we muſt put $A\,{}^{2}A^{2}$, as in the caſe of the regular octaedron.

33. A ſhort view of figure 23 will be ſufficient to give an idea of the method of denoting the regular hexaedral priſm in ordinary caſes; and with regard to the manner of placing the

the figures I ſhall not enlarge on it, as it may be eaſily deduced from what we have adopted in regard to quadrangular priſms. But, it ſometimes happens that three of the ſolid angles, taken alternately, are replaced by facets, while the intermediary angles remain untouched. In that caſe the expreſſion of the priſm will be that ſeen fig. 24.

34. The rhomboidal dodecaedron, in certain ſpecies, as that of red ſilver ore, has ſix of its faces which perform the functions of the planes of a priſm; while the ſix other faces enter into the analogy of rhomboids (29); ſo that the faces of each order may undergo particular decrements, independently of thoſe which regard the faces of the other order. Figure 25 repreſents the expreſſion of this dodecaedron. Each face of the ſummit, in the ſame caſe, may be conſidered as the baſe of an oblique quadrilateral priſm (20), and the adjacent planes as belonging to the ſame priſm. Thus the manner of placing the letters which indicate the decrements, and the figures that accompany theſe letters, will be analogous to that which takes place in quadrilateral priſms.

35. In other ſpecies, ſuch as that of the garnet and ſulphure of zinc, each ſolid angle, compoſed of three planes, may be aſſimilated to the ſummit of an obtuſe rhomboid; and thus by employing figures only for one face we ſhall have the expreſſion repreſented by figure 26.

36. We ſhall not employ the ſign of the dodecaedron with iſoſceles triangular planes, becauſe it is more natural to ſubſtitute the rhomboid from which it ariſes, as we have more ſimple laws of decrement.

37. It remains to give the means of repreſenting a particular caſe which takes place in certain cryſtals, where the parts oppoſite to thoſe that obey certain laws of decrement remain untouched, or are modified by different laws. This caſe belongs, in a particular manner, to turmalins; and it is eaſy then to indicate the difference by means of a zero. For example, in the very obtuſe turmalin, the nucleus of which we ſhall ſuppoſe repreſented figure 15, the priſm which is enneagonal has ſix of its planes produced by ſubtractions

tractions of one range on the ſix edges D, D, &c.; and the other three by ſubtractions of two ranges on three only of the angles E or *e*. Moreover, the inferior ſummit has ſimply three faces parallel to thoſe of the nucleus; while on the ſuperior ſummit the three edges B are each replaced by a facet, in conſequence of a decrement by one range which does not attain to its limits. The repreſentative ſign of this form will be: $\overset{1}{D}\ \overset{2}{e}\ \overset{2.0}{E}\ P\ B,\ b$. The quantities $\underset{1}{\overset{2.0}{E}},\ \underset{1.0}{b}$ ſhew, the firſt that the angles oppoſite to *e* undergo no decrement, and the other that the edges oppoſite to B remain alſo untouched. If theſe edges were ſubjected to a different law, taking place by two ranges, the ſign would become $\overset{1}{D}\ \overset{2}{e}\ \overset{2.0}{E}\ P\ \underset{1}{\overset{2}{B}}\ b$. According to this we are ſuppoſed to admit that the decrements, repreſented by a large letter, do not implicitly contain like decrements analogous to the ſmall letter of the ſame name; or reciprocally, that if the ſecond letter ſhould not enter into the expreſſion of the ſign with a different figure, we ſhould not place there the ſame figure accompanied with a zero. In the firſt caſe, each of the two letters expreſſes a decrement which is peculiar to the edges or angles it denotes; in the ſecond, that which is affected by a zero ſhews that the angle or edge it denotes undergoes no decrement.

38. Let us ſtill quote the variety of the ſulphure of zinc, which exhibits the dodecaedron having rhombuſes for its planes, the four ſolid angles of which, compoſed of three planes, are replaced by triangular facets ſituated like the faces of a tetraedron, while the oppoſite angles remain untouched *. By always adopting figure 16 to repreſent the primitive form, we ſhould thus expreſs the variety in queſtion: $\underset{1.0}{A}\ a\ \underset{1}{A}\ \underset{1.0}{a}$.

39. I have enlarged upon the explanation of the principles of this method, that I might, if poſſible, omit nothing

* This variety is ſtill modified by other facets, of which, for the greater ſimplicity, no notice is taken.

which

which may ſerve to give a clear idea of it, and enable an obſerver to repreſent immediately a ſecondary cryſtal of a given form. But if any one ſhould confine himſelf merely to a knowledge of the ſigns employed in this method, and ſhould only wiſh to read them, without being deſirous to know the art of writing them, a few ſimple rules only will be neceſſary to be known, which I ſhall here briefly mention; they will form a review of the preceding details.

1. Every vowel employed in the ſign of a cryſtal denotes the ſolid angle, marked with the ſame vowel on the figure repreſenting the nucleus; and every conſonant indicates the edge which bears that conſonant, or the face, the middle of which it occupies on the figure of the nucleus.

2. Each vowel and each conſonant is accompanied with one or more figures; the values of which, as well as the poſitions, indicate the laws of decrement which the angles or correſponding edges undergo. We muſt except the three conſonants P, M, T, each of which, when it forms part of the ſign of a cryſtal, indicates that the cryſtal has faces parallel to that which bears this letter.

3. Each letter, comprehended in the ſign of a cryſtal, is underſtood, with the cipher or ciphers that accompany it, on all the angles or edges, which perform the ſame functions as that which, in the figure, is immediately marked with the letter in queſtion.

4. Every whole number, placed above a letter, indicates a decrement in breadth, which aſcends in departing from the angle or edge marked with that letter.

5. Every whole number, placed below a letter, indicates a decrement which deſcends in departing either from the ſummit or the edge which bears that letter *.

* Alluſion is made here to the general progreſs of decrements, to which the particular caſes that ſeem to make an exception are referred. For example, if the decrement took place by one range on the angle at the ſummit of a rhomboid, the face produced would be horizontal; but this decrement enters into thoſe which are deſcending, and of which it is, as it were, the boundary.

6. Every

6. Every whole number, placed towards the top and on the right or left of a letter, denotes a decrement which takes place to the right or left of the angle, or of the edge marked with the ſame letter.

7. Every letter, ſuch as $^{3}H^{2}$ or $\overset{2.3}{G}$, which bears ſeveral figures, placed different ways or in the ſame manner, indicates that the correſponding edge or angle undergoes, at the ſame time, different kinds of decrements announced by the numbers.

8. The fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. which have unity for numerator, denote decrements in height by two, three, four ranges, &c.

9. The fractions $\frac{2}{3}$, $\frac{3}{4}$, $\frac{3}{2}$, &c. each term of which is greater than unity, denote mixed decrements by two ranges in breadth and three in height, or by three ranges in breadth and four in height, or by three in breadth and two in height.

10. The parentheſis, ſuch as $(\overset{3}{O}\, D^{1}\, F^{2})$, denotes an intermediary decrement. The letter $\overset{3}{O}$ indicates: firſt that the decrement takes place by three ranges on the angle O, and that its effect is aſcending. $D^{1}\, F^{2}$ make known that for one edge of moleculæ ſubtracted along the ſide marked D, there are two edges ſubtracted along the ſide marked F.

11. Every ſmall letter, comprehended in the ſign of a cryſtal, indicates the angle or edge diametrically oppoſite to that which bears the large letter of the ſame name on the figure; or the ſmall letter in queſtion is omitted as ſuperfluous.

12. We muſt except the letter *e*, which is always found on the figure of the rhomboid, and which indicates the angle oppoſite to that which bears the letter E.

13. When a ſign contains two letters of the ſame name, one large and the other ſmall, with different figures, the two angles or two edges oppoſite to which theſe letters correſpond, are ſuppoſed to undergo each ſeparately the law of decrement indicated by the figure that accompanies it.

14. Every

14. Every letter, whether large or ſmall, marked with a figure followed by a zero, makes known that the decrement indicated by this figure has no effect on the angle or edge to which that letter belongs.

X. *Curious Fact reſpecting the Natural Hiſtory of the Otter.* *By* C. POISSONNIER, *Juſtice of Peace of the Canton of Bonnat, Department of la Creuze.* *From* Le Moniteur Univerſel, *Nivoſe* 21, *An. VII.*

I HAD conſidered as a fable what Father Vaniere ſays, in the fifteenth book of his poem called *Prædium Ruſticum*, in regard to an otter which he had tamed to ſuch a degree that it would plunge before his eyes into a canal of vaſt extent, and bring to him with great fidelity the prey it had caught. From the accounts I had read in the works of different naturaliſts, I believed that this animal was of a nature ſo ferocious that it was no way ſuſceptible of being tamed; but I am now convinced of the contrary. Having procured a young otter in the month of Germinal laſt, it has fully repaid all the care and attention I beſtowed on it; for it goes regularly every morning to take a turn on the banks of the ſmall river Creuze, which runs at the diſtance of about a hundred paces from my habitation, and ſeldom returns without bringing me a fiſh ſtill alive. To whatever diſtance it goes, it always returns with the utmoſt punctuality to the ſmall kennel which I have conſtructed for it.

It has been ſaid alſo that this animal is amphibious, but I have found the contrary to be the caſe. My otter never plunges into the water but to catch its prey, and it returns as ſpeedily as poſſible to the banks, where it ſhakes itſelf like a ſmall water-ſpaniel. If it is obliged to remain under water for any length of time, it raiſes its head in order to breathe; from which I conclude that the otter ſwims better than other animals; but is not amphibious, ſince it cannot live under water above half an hour.

XI. *Letters*

XI. *Letters from ſome of the Men of Science engaged in the French Expedition to Egypt.*

LETTER I.

Cairo, Thermidor 25th, An. VI.

THE Commiſſion of the Sciences remained a month at Roſetta, until Egypt was entirely ſubdued. I am buſily employed at preſent in the department to which I belong. I have had the good fortune to be encouraged and protected by General Menou, who commanded in the province of Roſetta. He gave me an eſcort, to enable me to penetrate the Delta, and to hunt for animals. I have found a number of very intereſting birds. To obſerve them alive, deſcribe them zoologically and anatomically; to prepare ſtuffed ſkins and ſkeletons, have been my occupation in the moſt agreeable country of Egypt. I have made many new obſervations, which I intend to write down for the Inſtitute of Cairo.

The botaniſts are very unfortunate in regard to their ſcience. Egypt has ſcarcely furniſhed them with twenty different ſpecies; and, beſides, they have loſt all the paper which they brought with them. The caſe has been the ſame with my ſpirit of wine and powder for ſtuffing birds. Theſe articles were on board the *Patriote*, which was loſt in the harbour of Alexandria. This veſſel had on board alſo the effects of the aëronauts.

The moſt aſtoniſhing tree here is the ſycamore-fig. One of theſe trees is ſufficient to ſhade ſeveral of the peaſants' huts, with the oxen that raiſe water to water the meadows by means of wheels.

LETTER II.

Alexandria, Thermidor 25th.

THE naturaliſts are all employed each in his own department. The mineralogiſts have not found any thing of importance. They have been occupied only in examining the

the changes occaſioned in the land by the winds of Lybia. The ruins have furniſhed them a much more abundant harveſt. The thouſands of columns which every where occur have preſented them with granites, *breches**, and Egyptian marble of the greateſt beauty. The monuments, aſtoniſhing on account of their bulk, which are ſeen here, have already given us a ſpecimen of the wonders we ſhall behold in Upper Egypt. Pompey's pillar, and the column called Cleopatra's needle, do not, however, make ſo much impreſſion as an Egyptian vaſe covered with hieroglyphics, and in perfect preſervation. This vaſe ſtands in the great moſque of the Arab town; and I hope that you will ſee it in Paris, notwithſtanding its enormous weight.

I have examined with C. Champi the ſtones of which the walls of the Arab town are built. They are corroded to the very centre in ſuch a manner as might induce people to believe that ſaltpetre here is very abundant; but we were ſurpriſed to find ſcarcely any thing except ſea-ſalt and a very ſmall quantity of a nitrat, the nature of which we have not been able to determine, as we had not the neceſſary means, but which, in all probability, is nitrat of lime. I am employed in getting repaired the boxes which contain our chemical apparatus, and which have ſuffered conſiderably. More than half of the ſulphuric acid has been loſt, and has injured the boxes.

The aſtronomers are employed with the eſtabliſhment of a ſolid baſe, that is to ſay, with the conſtruction of two pillars of maſon-work. They will depart ſoon, in order to form the triangles neceſſary for making a map of the country. The military, civil, and geographical engineers are now conſtructing a plan of Alexandria and the environs.

Conté is employed in collecting ſpecimens of all the arts of the country. He has formed a plan alſo of a very ſimple

* The French give this appellation to a very ſhining hard kind of marble found in the Pyrenees. EDIT.

telegraph,

telegraph, to be eſtabliſhed on the coaſts and in the road to Cairo.

Alexandria ſtands in the middle of a deſert. The Turkiſh town is built at the expence of that of the Arabs, where nothing has been preſerved untouched but the ciſterns, none of which have been formed under the new town. The objects of culture here conſiſt of beautiful palms, which have a ſomewhat diſmal appearance, fig-trees, the *cactus opuntia*, and wretched vegetables, to which the inhabitants do much honour by giving them the names of cabbages, ſorrel, parſley, &c. The onions, however, deſerve particular attention. They are much harder, a little more pointed in their form, and have a ſomewhat ſtronger taſte than thoſe of Europe. The grapes which we have ate here for a fortnight paſt are brought by water from Roſetta and Cyprus: water-melons, which are abundant, come alſo from Roſetta: they are cultivated here, but in ſmall quantities.

The Bedouin Arabs who inhabit the deſert, and who feed there their flocks, which they afterwards bring for ſale to Alexandria, wear a white dreſs that appeared to me to have a great reſemblance to the ancient Roman habit, and which produces a very fine effect. The painters, when I aſked what they thought of it, entertained the ſame opinion. The men here are ſtrong, of large ſize, and well proportioned. This, no doubt, is owing to the dreſs worn by the children and the lower claſſes, which is merely a blue ſhirt. They take a great deal of exerciſe, and their ſtrength is expanded at a very early age.

We have been all indiſpoſed: this is a tribute we muſt pay to a climate ſo different from our own, and above all to the difference of nouriſhment. Though the heat here is only 22 or 23 (81 or 84 Fahr.), on account of the ſea-breeze which cools the atmoſphere, the hygrometric diſpoſition of the air has a powerful effect on the animal economy. It never rains at this ſeaſon, but in the evening there is an

abundant fall of dew, to which are aſcribed thoſe diſeaſes of the eyes that afflict the inhabitants.

The plague, ſo much dreaded in Europe, creates no uneaſineſs here, though it ſtill exiſts in ſome houſes of the city. People walk in the ſtreets with as much confidence as if they were not expoſed to the danger of touching an infected perſon, and catching that cruel malady. Dubois, the ſurgeon, has had a great deal of converſation with a phyſician who has long practiſed in this country. He aſſured him that the plague is not ſo dangerous as it is generally repreſented, and that many perſons, attacked by epidemic diſeaſes, die for want of aſſiſtance, becauſe the terror inſpired by the plague is ſtronger than all the ties of affection. A lazaretto was eſtabliſhed here almoſt as ſoon as we arrived. It is ſituated in the ancient iſle of Pharos, at the entrance of the old harbour.

[To be continued.]

XII. *On the Culture of Poppies and the Extraction of Opium in the Eaſt Indies.* *By* ARTHUR WILLIAM DEVIS, *Eſq.* *From the* Tranſactions of the Society for the Encouragement of Arts, &c. *Vol. XVI.*

I SEND for the acceptance of the Society a ſmall iron inſtrument, called, by the natives of Hindooſtan, Nehrea, and uſed by them for making inciſions in the capſules of the plants for the extraction of opium.

This inſtrument (the exact dimenſions of which are ſhewn Plate XI. Fig. 4,) is formed of two thin pieces of plate iron, ſharpened at the broad ſemicircular edge; and each being wound round with thread, the two are faſtened together by other thread wound round both *.

As the cultivation of the poppy in this country for the preparation of opium has lately engaged the attention of

* One of the inſtruments is reſerved in the Society's repoſitory.

this Society, I have taken the liberty to ſubjoin a few extracts from a memorandum on the ſubject, part of a work on the arts, manufactures, and agriculture, of Hindooſtan, collected at various times during my reſidence in India. I do not pretend to any practical knowledge; but if it may afford the leaſt hint to thoſe engaged in the purſuit, I ſhall feel great pleaſure in the communication.

The greateſt quantity of opium imported from India is prepared in the province of Behar, the ſoil of which moſtly conſiſts of clay, with a conſiderable portion of ſand. The richeſt ſoils of this deſcription are preferred by the natives for the cultivation of the poppy, which is the *Papaver ſomniferum* of Linnæus. The ground is ploughed ſixteen or eighteen times, and manured with the dung of cattle folded on it at night, or rotten cow-dung; the manure is ploughed in, and the ground thoroughly broken and carefully levelled with the harrow. The ſeed is ſown in the broad-caſt way; and the field is then divided into ſquares of four or five feet, leaving an interval, which is raiſed ſix inches; and on theſe ridges a channel is excavated to convey water to every ſquare, from a well near the field. The plants are five or ſix inches from each other, and are watered once a week, or oftener, until they are ſufficiently ripe for the extraction of the opium. About ſeven days after the flowers fall, when the capſules diſcover to the preſſure a degree of hardneſs, they begin to collect the opium. At ſun-ſet, a double inciſion is made on each capſule, from the ſtalk upwards, at one ſtroke of the nehrea: the operation is repeated every ſecond day, for a fortnight or three weeks. The juice which has exuded during the night, from the wounds made the preceding evening, is ſcraped off the capſule with a ſmall iron ſcoop; this is done early in the morning before the dew is diſperſed. The ſeeds are valuable for the oil they contain, and, as an article of food, are in great requeſt with the natives.

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XIII. *De-*

XIII. *Deſcription of the Mangle invented by Mr.* JEE. *Extracted from the* Tranſactions of the Society for the Encouragement of Arts, &c.* *Vol. XVI.*

THIS mangle (ſee Plate XI.) is ſo conſtructed that the handle is required to be turned one way only, which moves the machine more ſteadily and ſafer for the linen, than by varying the turning of the handle.

Fig. 1, is a perſpective view of the mangle when at work. The letters of reference anſwer to the ſame parts as in fig. 2; but here the great wheel is hid by a board which lies on the frame B.

A, fig. 2, is the great wheel, 15 inches in diameter, the teeth of which, being on the under ſide, cannot be ſeen in this view; B is the arbor on which the pinion C is faſtened, which is worked by the winch D. The pinion cauſing the large wheel to revolve, works the crank EF, (the part E is 21 inches long) which being faſtened to the moveable bed K by a pivot at G cauſes it to move forward and backward, by one continued motion of the winch, guided in its place by the pulleys IIII. HH are two levers for raiſing the bed to put in or take out the linen which is wound on the rollers LL: this is effected by the open ſtuds GG at the ends of the bed being brought alternately over the ends H of the levers, the other ends of which are then preſſed down by the hand.

Fig. 3 is a front view of one of the open ſtuds.

* In conſideration of the merit of this invention, the Society voted their ſilver medal to Mr. Jee. Two complete models are reſerved in their repoſitory for the uſe of the public.

XIV. *Communication from Dr.* THORNTON, *Phyſician of the General Diſpenſary, relative to Trials made with the different Factitious Gaſes.*

SIR,

Duke-ſtreet, Groſvenor-ſquare, Jan. 1, 1799.

THE introduction of PNEUMATIC MEDICINE promiſes, I think, much improvement to the *ars medendi*. Its effects deſerve, therefore, to be made public; and through the channel of your excellent Magazine, I ſhall lay before the public the reſult of numerous trials with the different gaſes, as experienced either by me or others.

Mr. Tilloch. R. J. THORNTON.

A Caſe of Dyſpepſia.

Mrs. Appleby, wife of the Rev. William Appleby, Wooten, Lincolnſhire, laboured under dyſpeptic complaints upwards of twelve years. Nine phyſicians had tried their art, but to no purpoſe. The diſorder ſeemed to gain ground, and was accompanied with violent ſpaſms and a paralyſis of her lower extremities. Dr. Harriſon of Horncaſtle, who laſt attended her, deſired ſhe would try the *oxygenating* ſyſtem; and conjointly with Dr. Darling of Glandford Bridge, it was effected in this manner. Bark and *oxymuriate* of potaſh were given, and *oxygen air* inhaled; and the change produced was ſo great, that to the ſurpriſe of the whole neighbourhood this lady was ſoon perfectly reſtored.

N. B. The Editor begs leave to inform the Public, that theſe intereſting communications will be *continued* regularly every month; and that Dr. Thornton would be obliged to any Gentleman for his correſpondence on the ſubject of the airs, which will be regularly tranſmitted to the Editor of the Philoſophical Magazine for publication.

INTEL-

INTELLIGENCE

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

FRANCE.

THE National Inſtitute, in its public ſitting Nivoſe 15, An. VII. propoſed the following prize ſubjects:

GRAMMAR.

This claſs not having received any paper that ſeemed worthy of the prize before announced, have prolonged the term of competition to the next year, and propoſe the ſame ſubject under the following form: "To examine what the French language has gained in clearneſs and elegance, and what it may have loſt in *naïveté* and energy ſince the time of Amyot to the preſent day."

The prize is a gold medal of the value of five hectogrammes. It will be adjudged in the public ſitting of Germinal 15, An. VIII. The papers muſt be written in French, and tranſmitted before the 1ſt Nivoſe, An. VIII.

This claſs alſo propoſed: "To point out the means proper for giving new activity to the ſtudy of the Greek and Latin languages in France."

The prize the ſame as the above, and will be adjudged in the public ſitting of Meſſidor 15, Year VIII. The memoirs muſt be tranſmitted before the 1ſt of Germinal the ſame year.

MATHEMATICS.

The Academy of Sciences had propoſed as the ſubject of the mathematical prize, for 1794, an inveſtigation of the orbit of the firſt comet of 1770. Aſtronomers have tried,

but without ſucceſs, to ſubject the obſervations made of that comet to the laws of parabolic motion. Proſperin and Pingré employed themſelves particularly in this reſearch, and mathematicians are indebted to Duſejour for an important piece of labour, in which he has reduced to leſs than a degree the *maximum* of the errors of the parabolic hypotheſis. Lexell was able to repreſent with preciſion theſe obſervations in an ellipſis deſcribed in the courſe of five years and a half; but as this comet was not ſeen before 1770, and has not appeared ſince, ſo rapid a revolution ſeems inadmiſſible; unleſs we ſuppoſe that the comet experienced, before its appearance, very ſtrong perturbations, which brought its aphelion much nearer to the ſun; and that, after its diſappearance, it experienced contrary perturbations, which removed that aphelion to a very great diſtance. We find, indeed, that, in the ellipſe alluded to, the comet paſſed very near to Jupiter at theſe two epochs. Theſe ſuppoſitions, however, are very improbable; and ought not to be admitted until we are well aſſured that the obſerved motion of the comet cannot be repreſented in a non-re-entering orbit (*orbite non rentrante*) or at leaſt one very much elongated.

To clear up this remarkable phenomenon in the ſyſtem of the world, the Inſtitute propoſes:

1. To diſcuſs all the obſervations which can be found of the comet of 1770, by determining, if neceſſary, the poſitions of the ſtars with which it was compared.

2. To examine, with care, whether the obſervations can be repreſented in a non-re-entering orbit.

3. Should that be found impoſſible, to determine the elements of the elliptic orbit, which correſpond moſt exactly with theſe obſervations. But to give to the calculations all the preciſion neceſſary, and to leave no doubt of the juſtneſs of the conſequences deduced from them, it will be abſolutely required that attention be paid to the following points:

1. To free the obſerved longitudes and latitudes from the

effects

effects of parallax, aberration and nutation, in order to obtain the true poſitions, and that there may be no need to employ, in the calculation of the geocentric places, but the longitudes counted from the mean equinox.

2. To employ in the calculus the true place of the ſun; that is to ſay, augmented 20 ſeconds for aberration, and counted from the mean equinox.

3. To give, for each obſervation, the difference of the obſerved place of the comet, and that deduced from calculation; and to join to theſe differences the corrections to be made in the heliocentric longitudes and latitudes, in order to make the error of the geocentric places vaniſh.

4. To give the error which muſt be produced on the geocentric place by an error of 10 ſeconds in the calculated longitude of the ſun; and to determine alſo the effect of a ſmall error in the radius vector of the earth.

5. Laſtly, though the nebuloſity which ſurrounds comets generally prevents their phaſes from being well diſtinguiſhed, it will not be uſeleſs to examine whether theſe phaſes could have had any ſenſible influence on the obſerved places; and, in that caſe, they muſt be taken into the account, if poſſible.

By paying attention to all theſe points, which, for the moſt part, have been hitherto neglected, it may be at length poſſible to diſcover whether the movements of that comet may not really be repreſented in a non-re-entering orbit, or whether the irregularities obſerved in its courſe may not have been owing, in a great meaſure, to the elements of the earth's orbit, which are not perhaps ſufficiently well determined to give with preciſion the geocentric places of a planet which approached ſo near the earth.

The prize is a kilogramme of gold. Papers on this ſubject will not be received beyond the 15th of Meſſidor, An. VIII. The Inſtitute will announce the piece which has gained the prize in its public ſitting of Nivoſe 15, An. IX.

PROCEEDINGS OF THE FRENCH NATIONAL INSTITUTE.

IN the above ſitting C. Paliſſot Beauvoir read a memoir on the rattle-ſnake. The author aſſerts that this reptile is not ſo terrible, nor of ſo ferocious a nature, as is commonly believed. There are even certain times of the year when it is extremely gentle, and will ſuffer itſelf to be laid hold of with the hand. C. Paliſſot caught nine rattle-ſnakes in the courſe of two hours, while hunting with an American, in the United States; and he ſaw one at the houſe of the ſame American, which had been kept there for five years. Some have pretended that the females devour their young; but C. Paliſſot ſays that they only place them in their mouth, and carry them in that manner when threatened with danger. As a proof of this, he relates the following curious anecdote: one day he ſaw a female rattle-ſnake accompanied by five young ones of about the ſize of the barrel of a quill. The young ones, frightened by the noiſe which he made, took ſhelter in the mouth of the mother, which carried them to a little diſtance. Having followed and concealed himſelf, the young recovered courage; quitted their retreat and began to ſport on the graſs, till, being alarmed by the noiſe which he again made, they returned to their former aſylum. This ſcene was repeated ſeveral times, and C. Paliſſot declares that different people told him they had been witneſſes of the ſame thing.

C. Peyre read a memoir on the danger to which the National Library is expoſed from fire, owing to the buildings in its vicinity.

C. Teſſier gave an account of the reſearches he had made on the ordinary and poſſible duration of geſtation or pregnancy. The author made a great many experiments on the females of different animals, ſuch as cows, mares, ſwine, rabbits, &c. In more than a hundred cows the time of geſtation varied from eight months twenty days to ten months ſeven days. Thoſe made on mares and rabbits

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gave variations equally great. In regard to the human ſpecies, the obſervations which ought to ſerve as certain data are very imperfect; but pregnancy, according to C. Teſſier, may be prolonged a quarter beyond the uſual time. A child, therefore, ſays the author, born beyond the eleventh month of the widowhood of the mother ought not to be any impeachment of her chaſtity. And ſuch was the ſentence paſſed by a celebrated decree of the parliament of Paris, which declared as legitimate a prince of Condé born eleven months after the death of his father *.

The ſitting was terminated by the reading of an allegorical poem in two cantos, by C. Colin-Harleville, entitled, The Adventures of Melpomene and Thalia.

INSTITUTE AT CAIRO.

THE National Inſtitute of Paris having received minutes of the eight firſt ſittings of the Inſtitute of Cairo, they were read to the three claſſes in their ſittings of the 26th, 27th, and 28th of Frimaire.

The Inſtitute of Cairo, formed by Bonaparte after the model of that of France, conſiſts of four claſſes: mathematics, the phyſical ſciences, political economy, literature, and the

* An inſtance of the ſame kind, which occurred at Rome, is related by Aulus Gellius. A woman of a reſpectable character brought forth a child in the eleventh month after the death of her huſband. The woman was accuſed of having violated her chaſtity; but the emperor Adrian, under whoſe reign the affair happened, decreed that it was poſſible for pregnancy to be extended to that period. The following are the author's words: Præterea ego de partu humano, præterquam quæ ſcripta in libris legi, hoc quoque veniſſe uſu Romæ comperi. Fœminam bonis atque honeſtis moribus non ambigua pudicitia, in undecimo menſe, poſt mariti mortem, peperiſſe; factumque eſſe negotium propter rationem temporis, quaſi marito mortuo poſtea concepiſſet, quoniam decem viri in decem menſibus gigni hominem, non in undecimo ſcripſiſſent: ſed divum Hadrianum, cauſa cognita, decreviſſe in undecimo quoque menſe partum edi poſſe: idque ipſum ejus rei decretum nos legimus. In eo decreto Hadrianus id ſtatuere ſe dicit requiſitis veterum philoſophorum et medicorum ſententiis. *A. Gell. Noct. Att.* lib. iii. cap. 16. EDIT.

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fine arts. The members who compoſe the firſt claſs are: Andreoſſi, Bonaparte, Coſtaz, Fourier, Girard, Lepère, Leroi, Malus, Monge, Nouet, Queſnot, Horace Say. Thoſe which compoſe the phyſical claſs are: Berthollet, Champy, Conté, Deliſle, Deſcotils, Deſgenettes, Dolomieu, Dubois, Geoffroy, Savigny. Thoſe who compoſe the claſs of political economy are: Caſarelli, Gloutier, Pouſſielgue, Sulkowſky, Sucy, Tallien. Thoſe who compoſe the claſs of literature and the arts are: Denon, Dutertre, Norry, Parſeval, Redouté, Rigel, Venturi, D. Raphael.

The objects with which the Inſtitute will be occupied are: 1. The propagation of knowledge in Egypt. 2. Reſearches into the natural hiſtory, induſtry and hiſtory of Egypt. The Inſtitute, which meets on the firſt and ſixth of every decade, held its firſt ſitting on the 6th of Fructidor, An. VI. In this ſitting Monge was appointed preſident, and Bonaparte vice-preſident. General Bonaparte then propoſed the ſix following queſtions: 1. Are the ovens employed for baking ammunition bread capable of any improvement in regard to the expence of fuel, and what are theſe improvements?

2. Does Egypt furniſh any thing which can be ſubſtituted for hops in the brewing of beer?

3. What are the beſt means to cool and purify the water of the Nile?

4. In the preſent ſtate of things at Cairo, whether is it moſt convenient to conſtruct a wind-mill or a water-mill?

5. Does Egypt furniſh reſources for manufacturing gunpowder, and what are theſe reſources?

6. What is the ſtate of juriſprudence, of judiciary order civil and criminal, and of education in Egypt? What improvements are poſſible in theſe departments, and wiſhed for by the natives?

Second Sitting, Fructidor 11.

C. Andreoſſi made a report on the fifth queſtion propoſed by

by Bonaparte in the preceding ſitting. He firſt obſerved, that Egypt does not furniſh ſulphur, and that this article was formerly procured from Venice. The Commiſſion were of opinion that it might be procured from Sicily. The charcoal employed in Egypt is from the lupine, which is burnt in trenches and afterwards ſifted. The ſaltpetre is indigenous; and it is even ſaid that veins of it are found in the neighbourhood of Cairo: it is manufactured in the ſame manner as in Europe. It is there in the ſtate of a real nitrat of potaſh, and not of lime, as in France: it is baked with the ſtraw of Turkey corn, and purified with the white of an egg. Gunpowder is manufactured with the hand, and the workmen labour quite naked. Each mortar contains fifteen pounds, and it is pounded for ſeven hours. The peſtles weigh from nineteen to twenty-five pounds each. The wages of the workmen are from twenty to twenty-five paras: they add water to it, and to granulate the powder it is ſifted. This powder is excellent, and cheaper than that made in France. The inhabitants manufactured 2000 cantars annually, a great deal of which was exported to Leghorn. The beys had very little powder. Mourad-bey had no more than 1500 cantars. This manufactory might with care be eaſily increaſed, and conſiderable quantities of gun-powder might be ſent from Egypt to Europe.

Cit. Monge read a memoir on that optical phenomenon called by the French ſailors *mirage*.

It often happens at ſea, that a ſhip ſeen at a diſtance appears as if painted in the ſky, and not to be ſupported by the water. A ſimilar effect was obſerved by the French in the courſe of their march through the Deſert: the villages ſeen at a diſtance ſeemed to be built on an iſland in the middle of a lake. In proportion as they approached, the apparent ſurface of the water became narrower; when they were only at a ſmall diſtance, it diſappeared, and the ſame illuſion began, in regard to the next village. C. Monge aſcribes this effect to a diminution of the denſity of the lower ſtratum of the atmoſphere.

moſphere. This diminution in the Deſert is produced by the increaſe of heat ariſing from that communicated by the rays of the ſun to the ſand with which this ſtratum is in immediate contact. At ſea it takes place when, by particular circumſtances, ſuch as the action of the wind, the lower ſtratum of the atmoſphere holds in ſolution a greater quantity of water than the other ſtrata. In this ſtate of things the rays of light, which come from the lower parts of the heavens, having arrived at the ſurface that ſeparates the leſs denſe ſtratum from thoſe above it, do not paſs through that ſtratum, but are reflected, and paint in the eye of the obſerver an image of the heavens, which appearing to him to be below the horizon, he takes it for water, when the phenomenon occurs at land. If he is at ſea, he thinks he ſees in the heavens all the objects which float on that part of the ſurface occupied by the image of the heavens.

Third Sitting, Fructidor 16.

C. Berthollet read a memoir on the formation of ammonia under ſeveral circumſtances where the exiſtence of it has not hitherto been ſuppoſed.

C. Sulkowſky read a deſcription of the route from Cairo to Salehié. The route which the French army followed in purſuing Ibrahim Bey had been before unknown. Thoſe who proceed from Cairo by the gate of Naſr firſt enter the Deſert where there are a great many houſes now deſerted. The village of Elmalarié, which is found on this route, was the ancient Heliopolis. In another village called Elmare there are thouſands of palm trees. To the right of this route there is nothing except ſandy deſerts; but on the left there is abundance of cultivated land. The places through which the army paſſed were Lacoubey, Elhanea, Elmenié, Belbeys, Souva, Coraim, Salehié, and a great many others, which it only traverſed in the courſe of a rapid march.

C. Berthollet communicated the reſult of his examination of the gun-powder found in the caſtle of Cairo. This pow-

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der contained only $\frac{5}{3\frac{1}{2}}$ ($2\frac{1}{2}$ ounces per pound) of ſaltpetre; the reſt was ſulphur, charcoal, earth, and muriat of ſoda.

C. Monge read a memoir on ſome ancient monuments found at Cairo. We learn by this memoir: 1. That at a place called the *Sazar*, in a receſs incloſed by a grate ſituated in the ſtreet conducting from the building where the Inſtitute ſits to the caſtle of Cairo, there was a granite vaſe, covered with hieroglyphics both within and without. This vaſe had ſome reſemblance to one found in the great moſque at Alexandria, but the dimenſion of the hieroglyphics was larger. C. Monge requeſted that the Inſtitute would take the neceſſary meaſures to get this monument removed to a proper place, till an opportunity could be found of ſending it to France. 2. That in the caſtle of Cairo, after having paſſed the place called Joſeph's Caſtle, there was a door-threſhold, formed of a fragment of a poliſhed obeliſk of baſaltes, the hieroglyphics of which were in good preſervation. C. Monge made a ſimilar requeſt in regard to this monument.

C. Monge preſented to the aſſembly a ſpecimen of the ſtone employed for the maſon-work of the caſtle of Cairo. This ſtone is of the ſame nature as the rock on which the caſtle is built, and is compoſed of that ſhell called *numiſmal*, becauſe it reſembles ſmall pieces of money. Theſe ſhells may be eaſily ſplit in the direction of their thickneſs. In the inſide of them is ſeen a delicate ſpiral, which, at different points of its courſe, divides itſelf into ſeveral branches. This ſpiral ſerved as a lodging for the animal which conſtructed the ſhell. The old walls of Laon, in the department of Aiſne, are of a compoſition perfectly ſimilar.

Fourth Sitting, Fructidor 21.

C. Sulkowſky informed his colleagues that he had obſerved a buſt of Iſis on the banks of the Nile near Terané, and two ſtones with hieroglyphics in a garden of the ſame village. He requeſted that they might be tranſported to Cairo, and depoſited in the place where the Inſtitute met.

C. Geoffroy

C. Geoffroy read a memoir, from which it refults that the oftrich exhibits only the rudiments of that organifation neceffary for flying. All the apparent inftruments of flying in that bird are conftructed in fuch a manner as to preclude the poffibility of it. Extent of wing and confiftence of feathers are not the only things wanting. It is not provided with mufcles fufficient to raife it and fupport it in the air, and its air veficles are of lefs fize than thofe found in other birds.

Fifth Sitting, Fructidor 26.

A commiffion having been appointed to make a report on the fuel moft advantageous to be ufed for heating the camp-ovens, and on the means to reduce the confumption, C. H. Say prefented that report, the refult of which was, that the ftalks of the carthamus or fafranum, reeds, and the ftraw of maize, would furnifh a fufficiency of fuel to heat the ovens, and at a moderate expence.

C. Bonaparte prefented to the Inftitute a copy of the *Connoiffance des Temps* for the year VII; and invited the Inftitute to employ themfelves in the formation of an almanack. C. Monge, Nouet, Dom Raphael, and Beauchamp were charged with this object. This almanack was to comprehend a triple divifion of time according to the cuftom of the French, that of the Cophts, and that of the Muffulmans.

C. Fourier read a memoir on the general folution of algebraic equations.

C. Parfeval read a tranflation of a fragment of Taffo taken from the 17th canto.

C. Defgenettes read fome obfervations on various difeafes which ill-informed perfons might confound with the plague, and from which he proved them to be very different.

Sixth Sitting, 1ft Comp. Day, Year VI.

C. Beauchamp prefented a fhort calendar, which might be immediately printed for the ufe of the army. This calendar contained the old and the new divifion of time. C. Beauchamp

Beauchamp communicated alſo ſeveral aſtronomical obſervations, which are to be publiſhed in the Memoirs of the Inſtitute.

C. Berthollet read a letter addreſſed to him by C. Laplace, member of the French National Inſtitute. That geometrician recommended to him to make obſervations of the occultations of the ſtars by the moon. The ſame member gave an account alſo of the proceſſes employed in Egypt for making indigo. They are exceedingly ſimple, but imperfect. It appeared, however, that by means of ſome ſlight changes the quality of this article might be much improved.

C. Fourier read a note on the plan of a machine moved by wind, which might be employed for watering land. The wheel expoſed to the action of the wind is horizontal. All its parts are fixed, and the wings are ſo ſituated that they always turn in the ſame direction, whatever be the direction of the wind.

Seventh Sitting, Vendemiaire 6, An. VII.

C. Norry read a memoir concerning the column of Alexandria called Pompey's Pillar, in which he gave the dimenſions of the principal parts of that monument. On this occaſion C. Dolomieu offered an opinion reſpecting the period when this column was erected, according to which it ought to be referred to the age that followed the reign of Conſtantine. The chapiter and pedeſtal exhibit the character of degradation, which diſtinguiſhes the architecture of that time; but the ſhaft ſeems to belong to an earlier period, when that art ſtill retained all its purity.

C. Savigny read a memoir on a new ſpecies of *nymphæa*. The author gave an account of the characters common to that plant and thoſe which bear the ſame name, and pointed out the differences.

C. Dutertre read a memoir on the eſtabliſhment of a ſchool of drawing. His propoſal was referred to a commiſſion compoſed of C. Denon, Rigel, Dutertre, Redouté, Sucy and Norry.

C. Coſtaz

C. Coſtaz read a memoir, in which he explained the variations in the colour of the ſea.

C. Parſeval read a poetical tranſlation of a fragment of the 16th canto of Taſſo's Jeruſalem Delivered.

Eighth Sitting, Vendemiaire 11.

Fifty mummies of birds ſent to the Inſtitute were delivered to a commiſſion compoſed of Bonaparte, Geoffroy, Dolomieu and ſome others, in order to be examined.

Porte, a French inhabitant of Cairo engaged in the manufactory of indigo, preſented ſpecimens of it to the Inſtitute.

C. Larrey communicated a memoir on opthalmiæ.

C. Beauchamp read a memoir on his voyage from Conſtantinople to Trebiſonde.

C. Deliſle read a memoir on the palm which bears the fruit called *domm*. It is the *cuſſiophora* of Theophraſtus.

C. Dolomieu read a memoir, in which he ſhewed the neceſſity of ſtudying ancient geography and geology. He fixes the ſite of the ancient Alexandria between two hills of ſandy calcareous ſtone. He explained the ſucceſſive changes which that city experienced, and is of opinion, that the ſea muſt have riſen a foot ſince the time of Ptolemy.

The above pieces when delivered were accompanied by the firſt number of a new Journal entitled, LA DECADE EGYPTIENNE, *Journal littéraire et d'Economie politique, An.* VII. *de la Republique Françaiſe,* 10 *Vendemiaire.*

This journal is publiſhed every tenth day. Each number conſiſts of two ſheets and a half octavo, and coſts 20 ſous French money, or 10 francs for 12 numbers. Subſcriptions received by C. Marc-Aurele, printer to the army, French quarter at Cairo. It was accompanied by a proſpectus announcing that this journal would be merely literary; that no intelligence or political diſcuſſions would be admitted; but that every thing relating to the arts and ſciences, commerce, both in a general and particular point of view, civil and criminal legiſlation, moral and religious inſtitutions, would be thankfully received.

MISCELLANEOUS.

COATING FOR GLASS RETORTS, &c.

Profeſſor Wurzer at Bonn, in a letter to Profeſſor Götling of Jena, ſays: "It is not merely economy that makes us deſirous of obtaining good coating for glaſs apparatus, but becauſe the labour beſtowed on many proceſſes is often rendered entirely uſeleſs by the veſſel not being able to ſtand the neceſſary degree of heat. As circumſtances during a certain time prevented me from obtaining new glaſs apparatus, I made various trials to ſupply that deficiency, and found the following coating of the greateſt ſervice. I took fragments of porcelain coarſely pulveriſed and well ſifted, and as much pure clay, which I previouſly ſoftened with as much of a ſaturated ſolution of muriat of ſoda as was requiſite to give the whole the proper conſiſtence, and then proceeded in the uſual manner. My glaſſes," adds Profeſſor Wurzer, "which are coated with this ſubſtance, often ſtand an incredible degree of heat without breaking."

FREEZING OF MERCURY.

The ſevere cold which was experienced in London at Chriſtmas afforded an opportunity to Mr. Pepys junior, and ſeveral other able chemiſts, to repeat the experiments of Mr. Lowitz, of Peterſburgh, on the production of extraordinary cold. The reſult confirmed the truth of them. When the thermometer was at 17 of Fahr. a mixture of ſnow and muriat of lime produced ſuch a degree of cold as to freeze a quantity of mercury in a few minutes. It was then malleable, and, when broken, exhibited a fracture ſimilar to that of zink.

At the ſame time the chemiſts in Paris were occupied in repeating Lowitz's experiments. C. Fourcroy and Vauquelin by means of the ſame mixture froze twenty pounds of mercury in a platina crucible in thirty ſeconds: mercury in

a porcelain crucible took four times as long to freeze. Upon inſerting the end of the finger in the mixture, it loſt, in four ſeconds, all feeling, and was not reſtored to ſenſation till after it had been held a long time in the mouth. Upon the firſt inſertion of the finger in the mixture, an acute pain was felt, as if it had been violently preſſed in a vice.

The extreme cold by our accounts from the Continent was very general over Europe.

NEW VOYAGES OF DISCOVERY.

The French Government have in contemplation a new voyage of diſcovery round the world, under the direction of Captain Baudin, who lately returned from a botanical expedition to America. Three corvettes, *le Vengeur*, *la Serpente*, and *la Menaçante*, are fitted out for this purpoſe, and will ſail as ſoon as paſſes can be procured from the Britiſh Miniſtry. They are to proceed firſt to Teneriffe, for the purpoſe of collecting plants, and thence along the coaſt of Africa to the Cape of Good Hope. They are then to ſail along the eaſtern ſide of Africa, from which one of the corvettes is to be ſent back to Francewith the plants, that they may not be ſpoiled by a longer voyage. The other two corvettes will then proceed to New Holland, to make geographical obſervations on the unknown parts of that iſland; and afterwards viſit the coaſt of Peru, Chili, &c. and particularly the river La Plata, which they are to ſail up as far as they poſſibly can.

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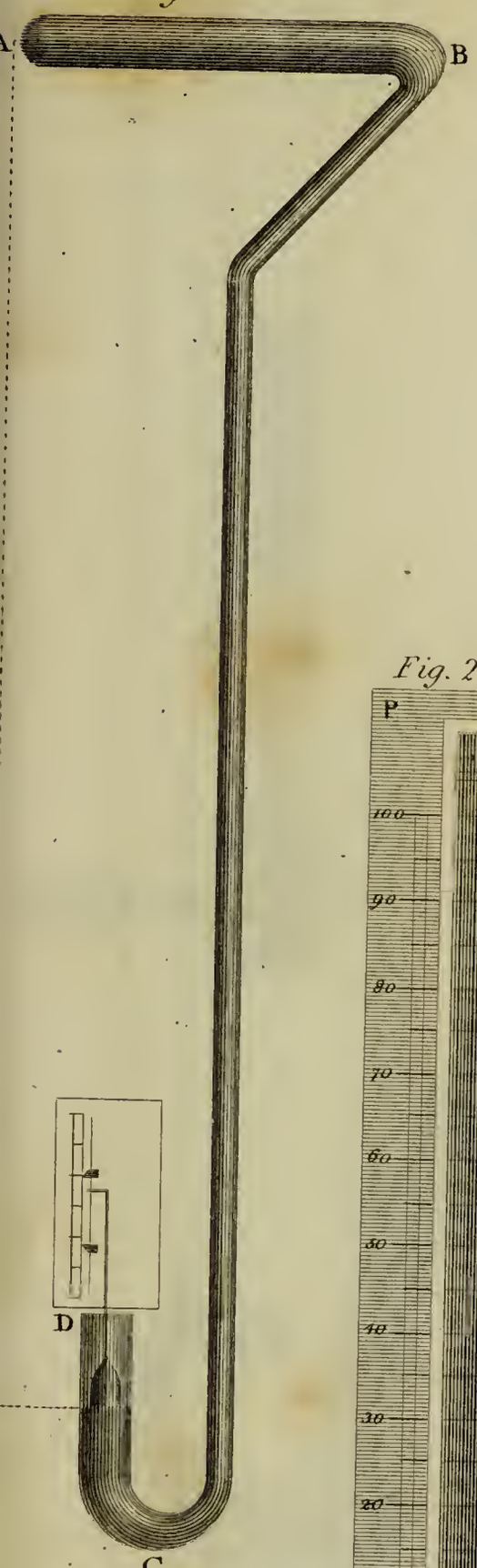

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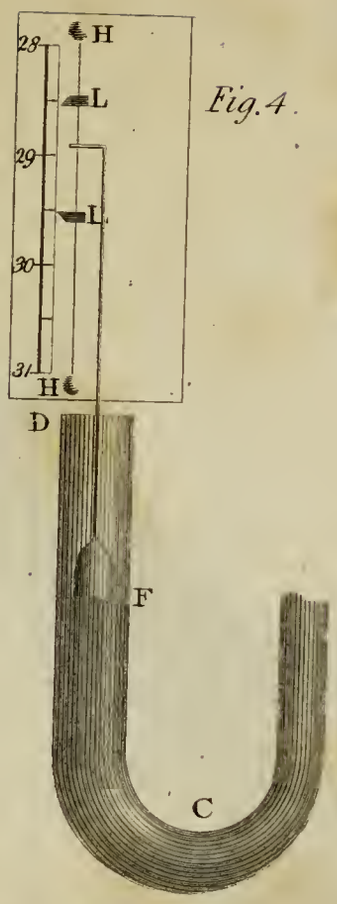

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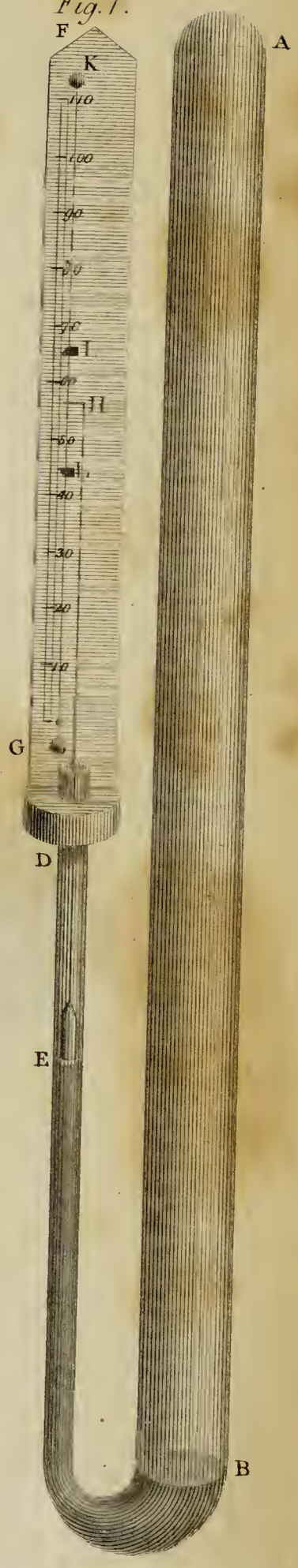

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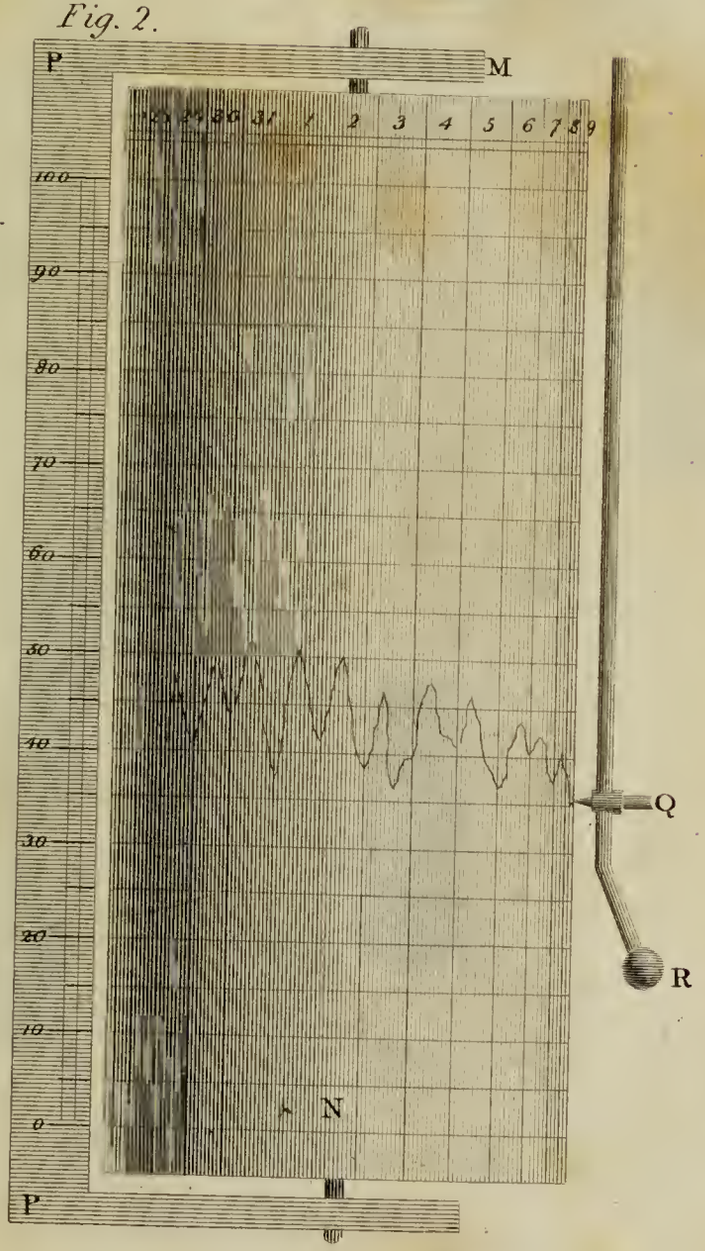

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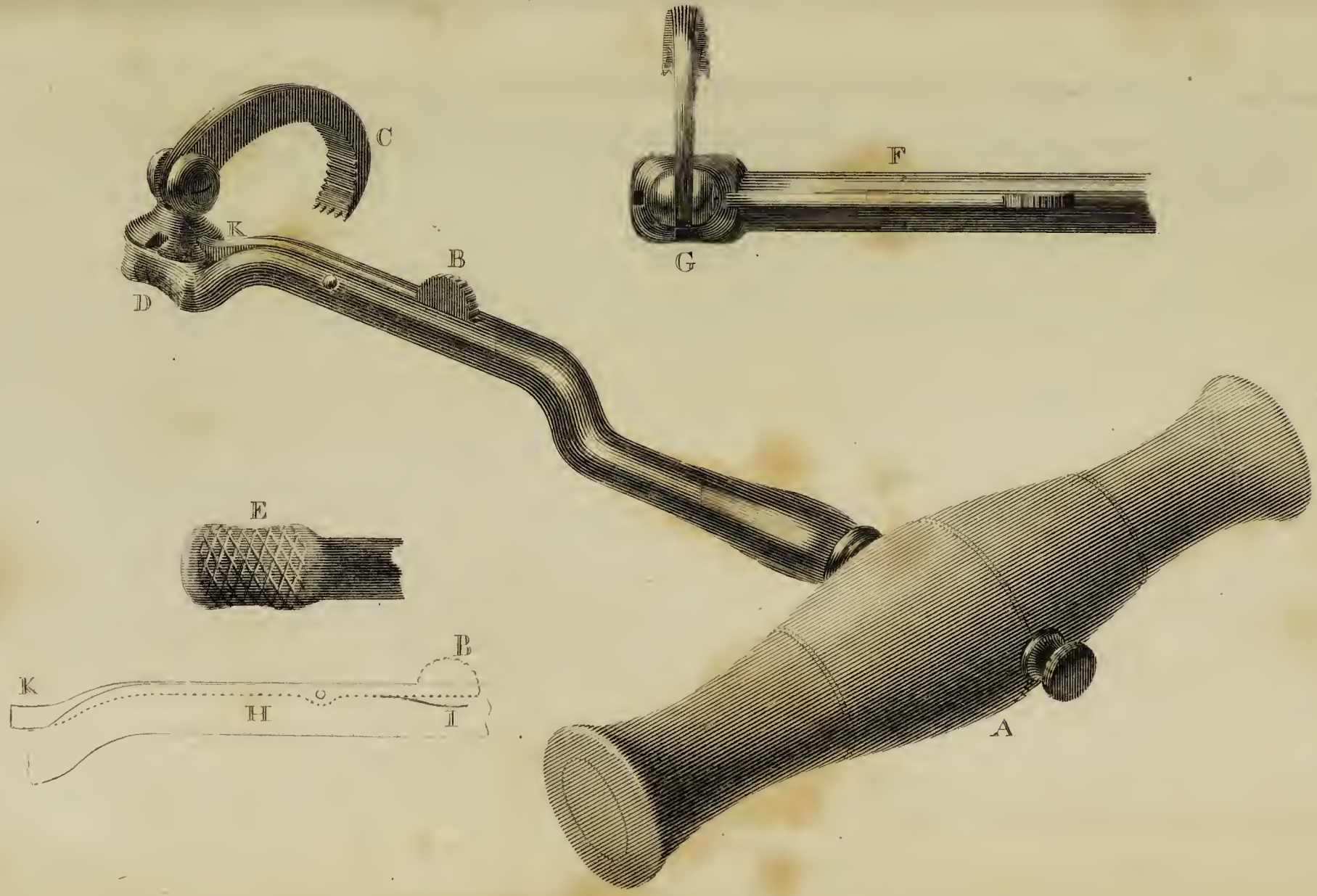

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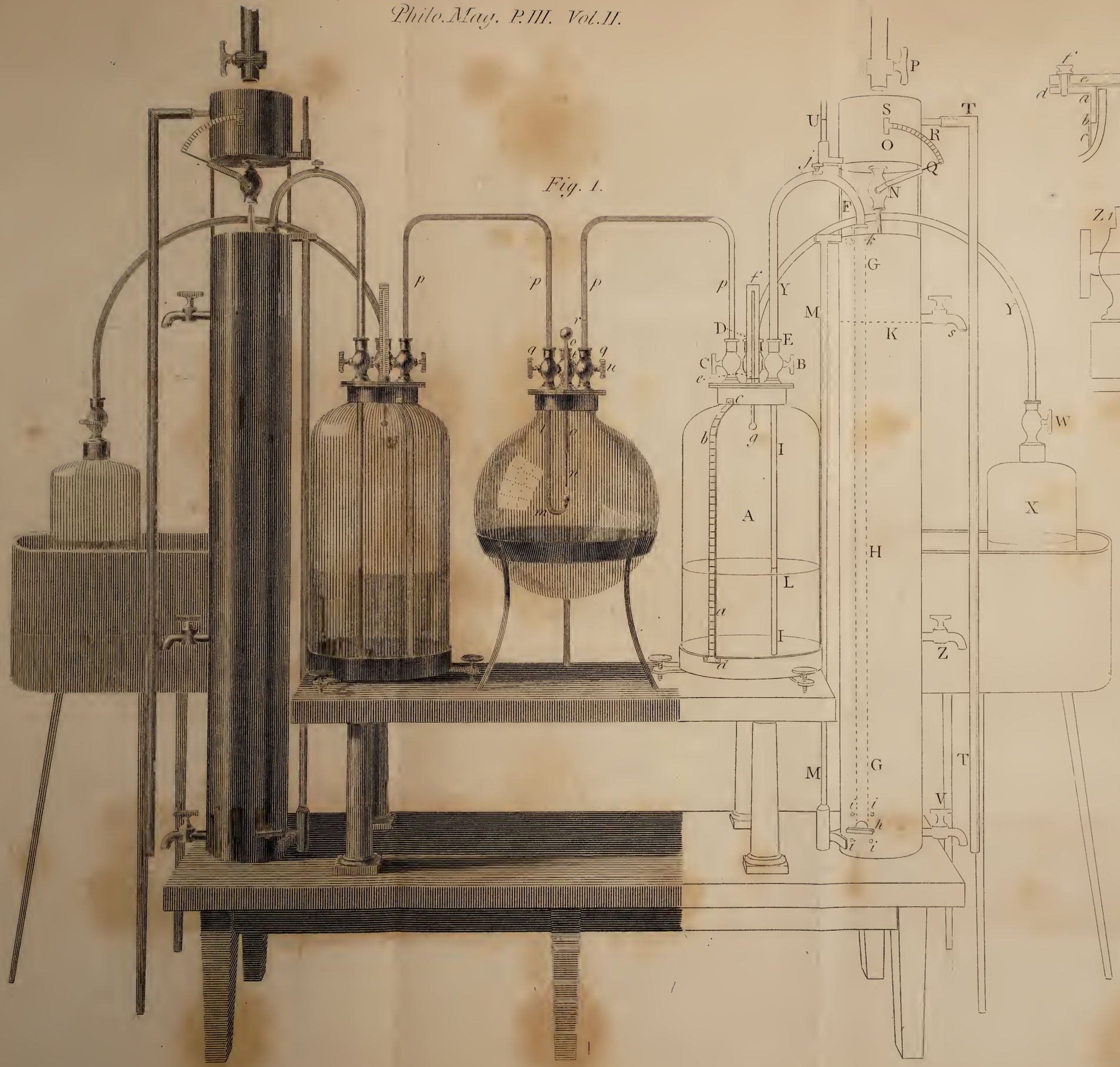

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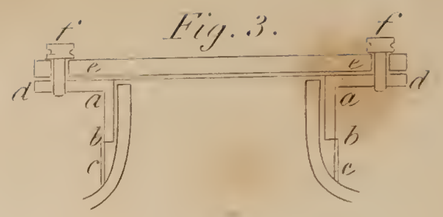

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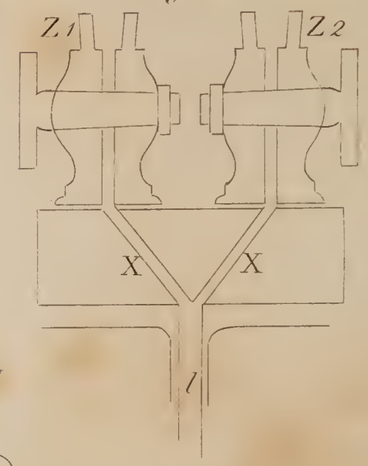

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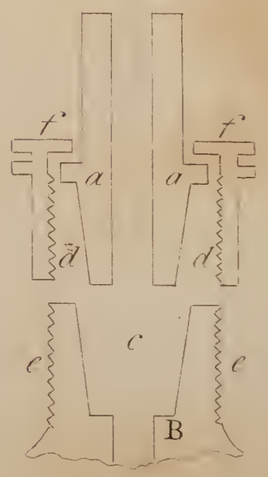

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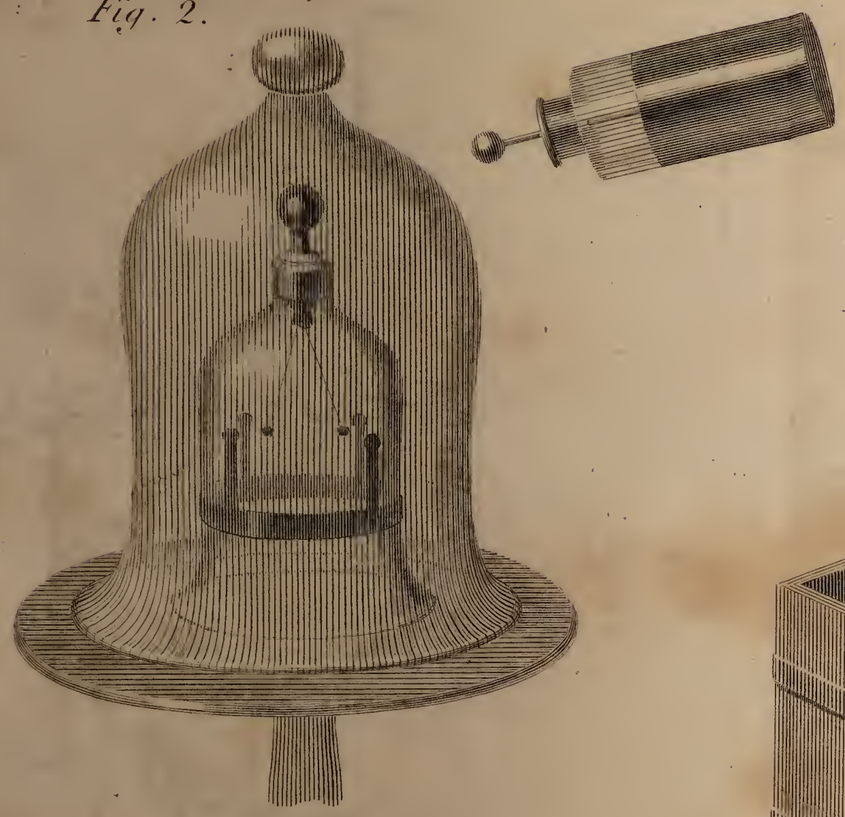

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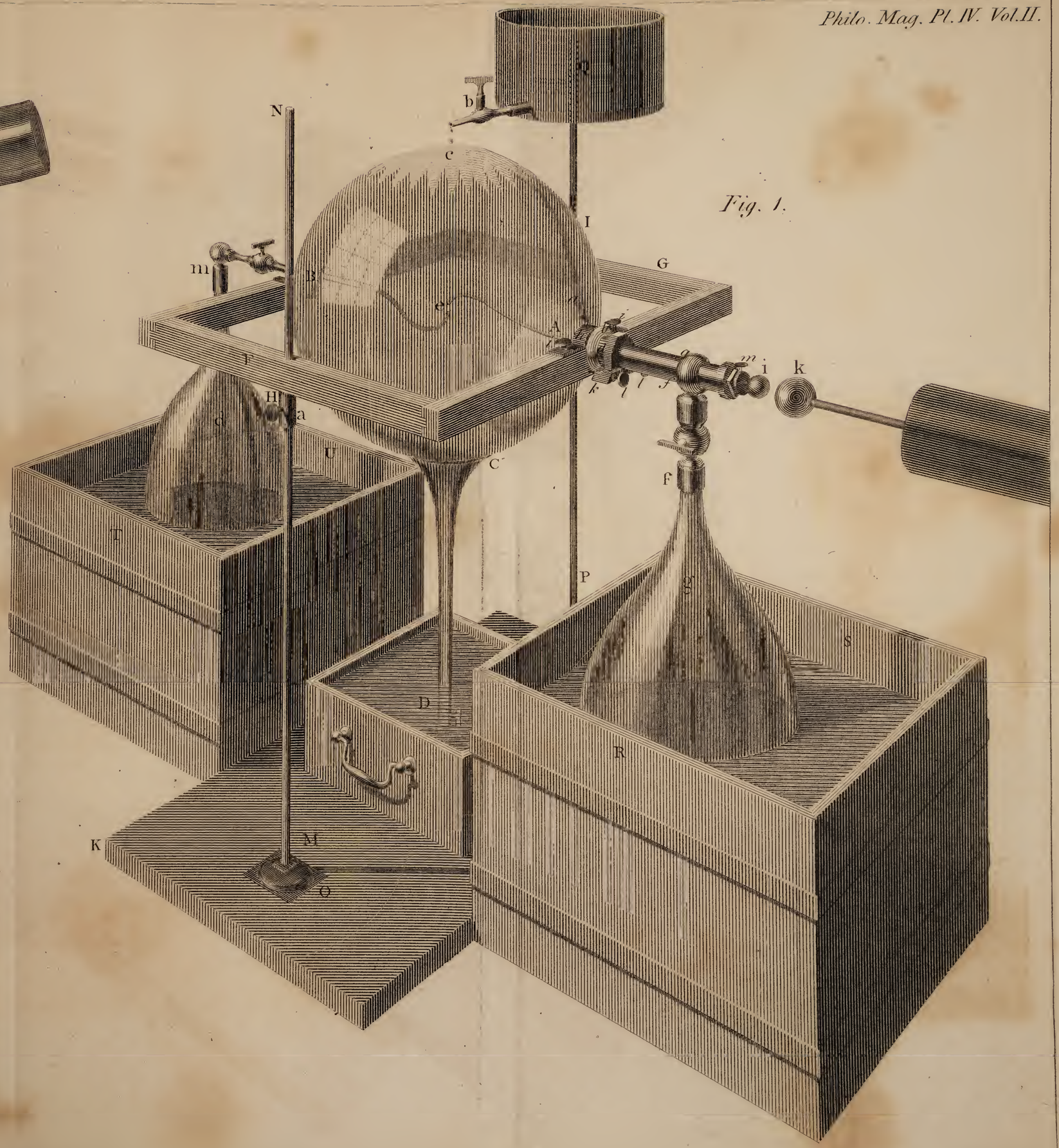

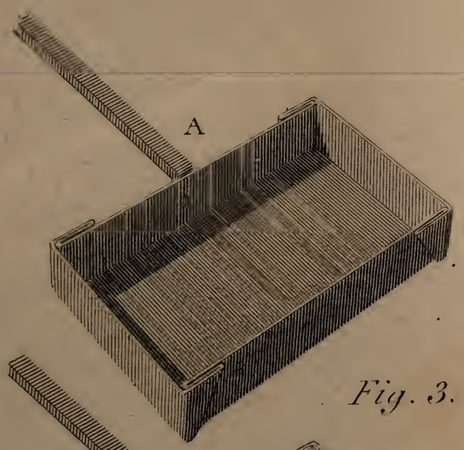

Fig. 3.

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Irish Wolf Dog.

Wild Ass of Middle Asia.

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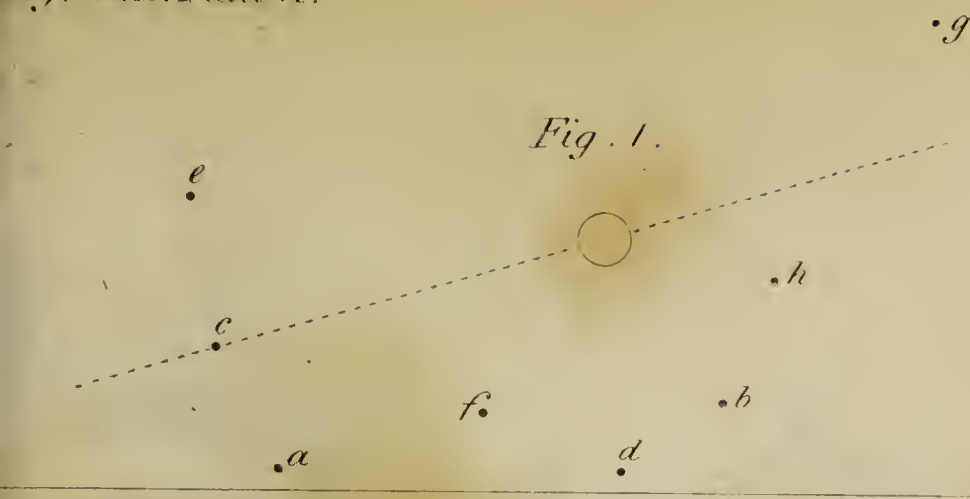

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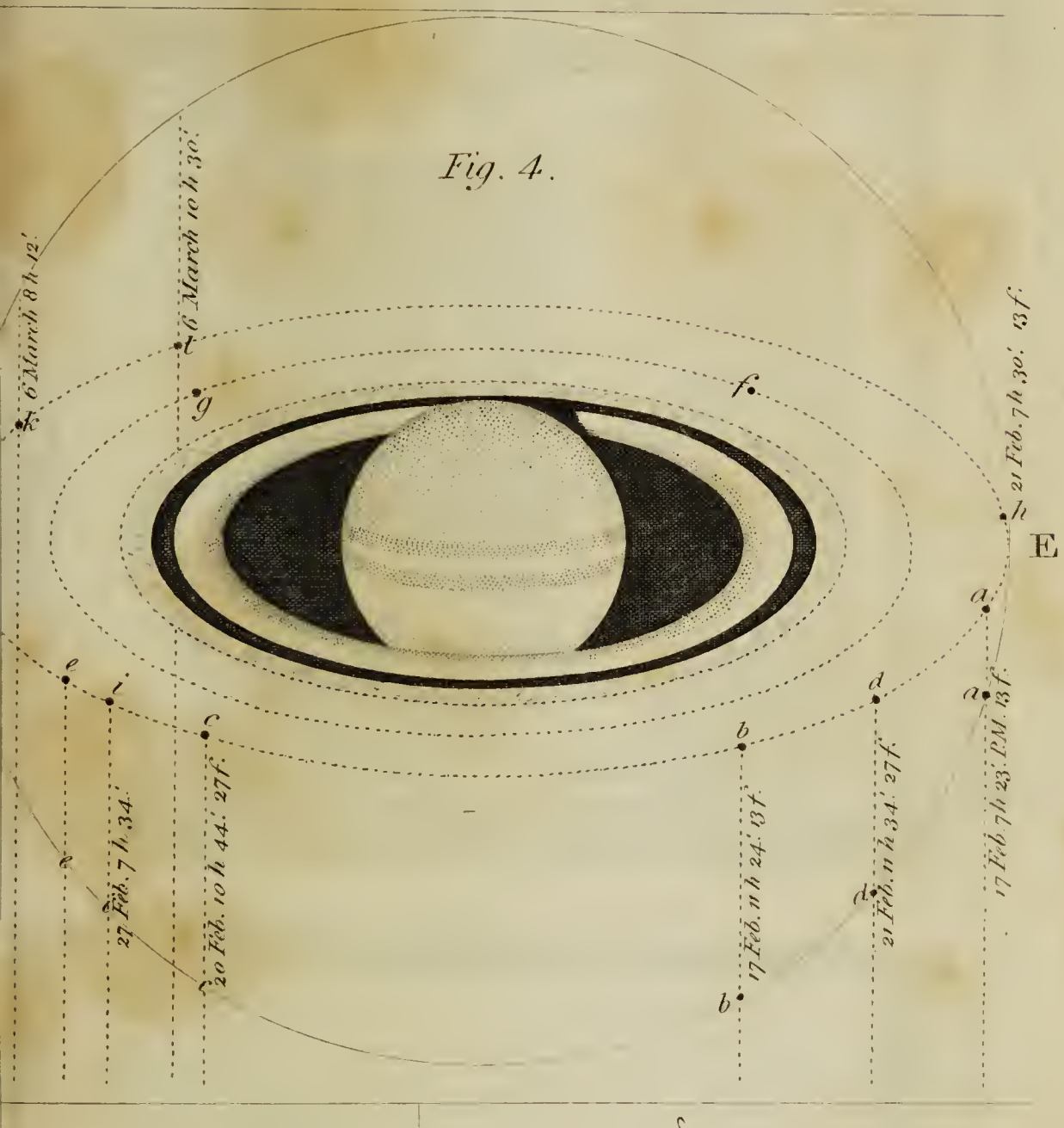

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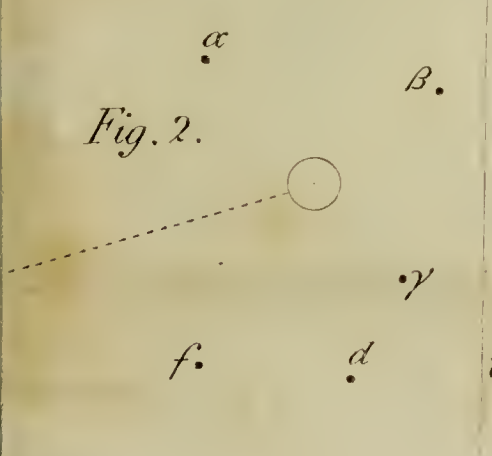

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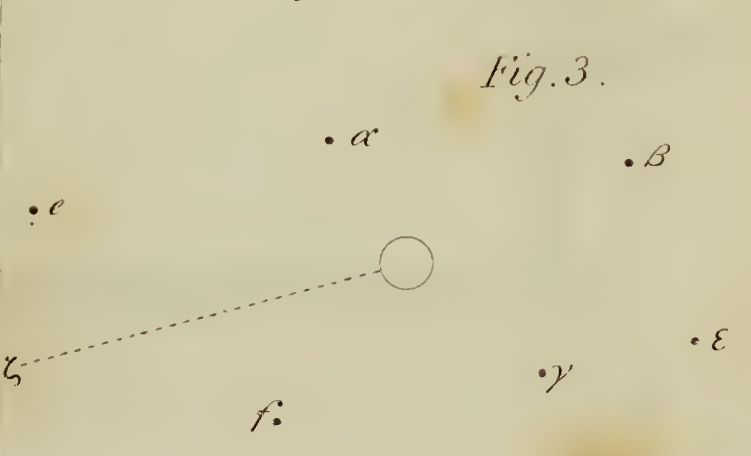

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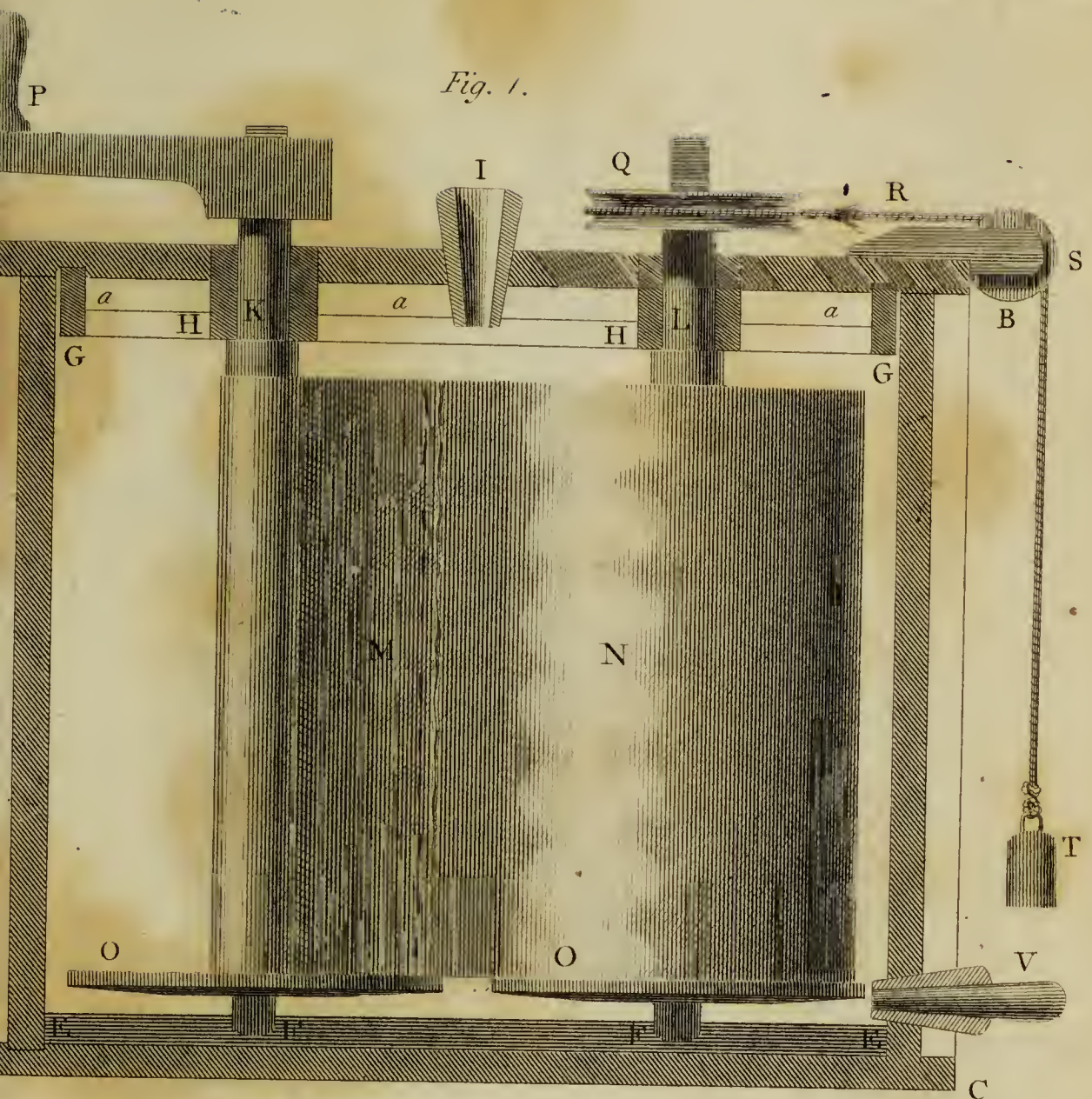

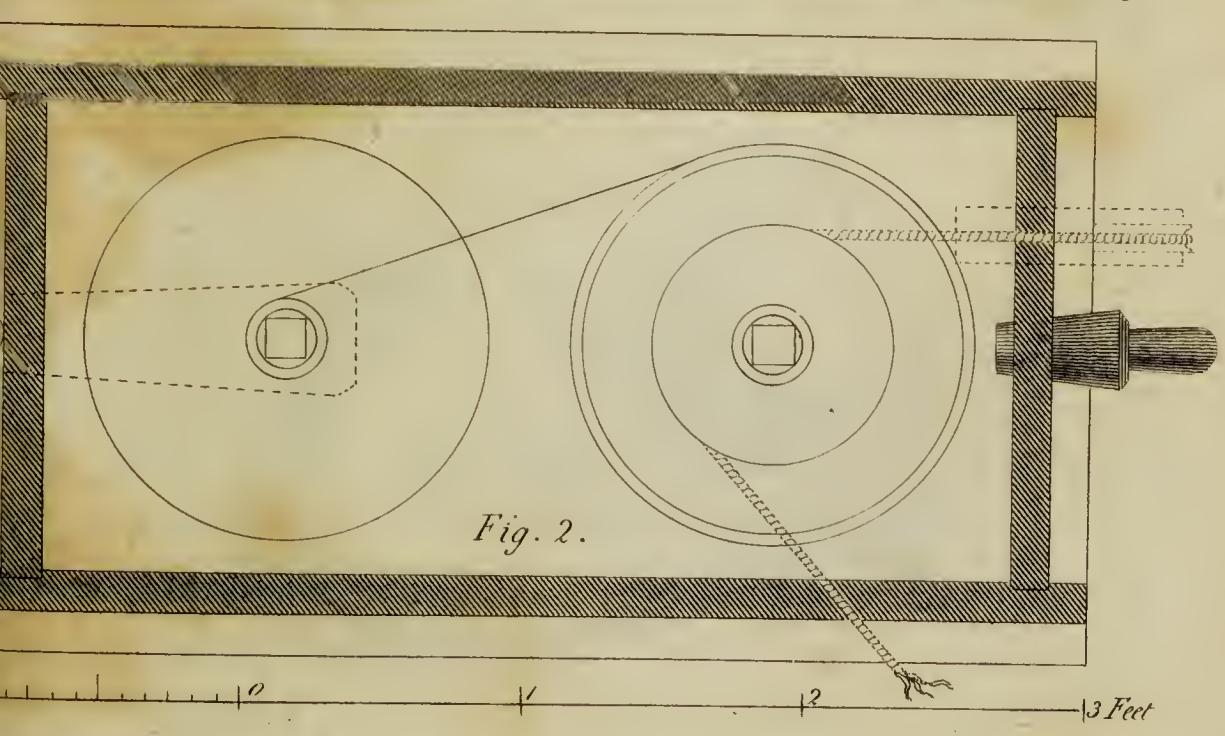

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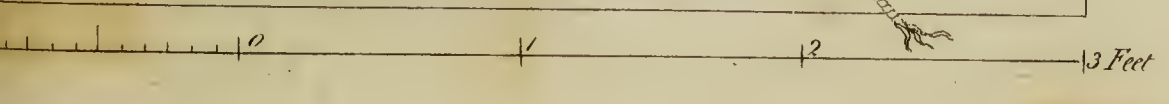

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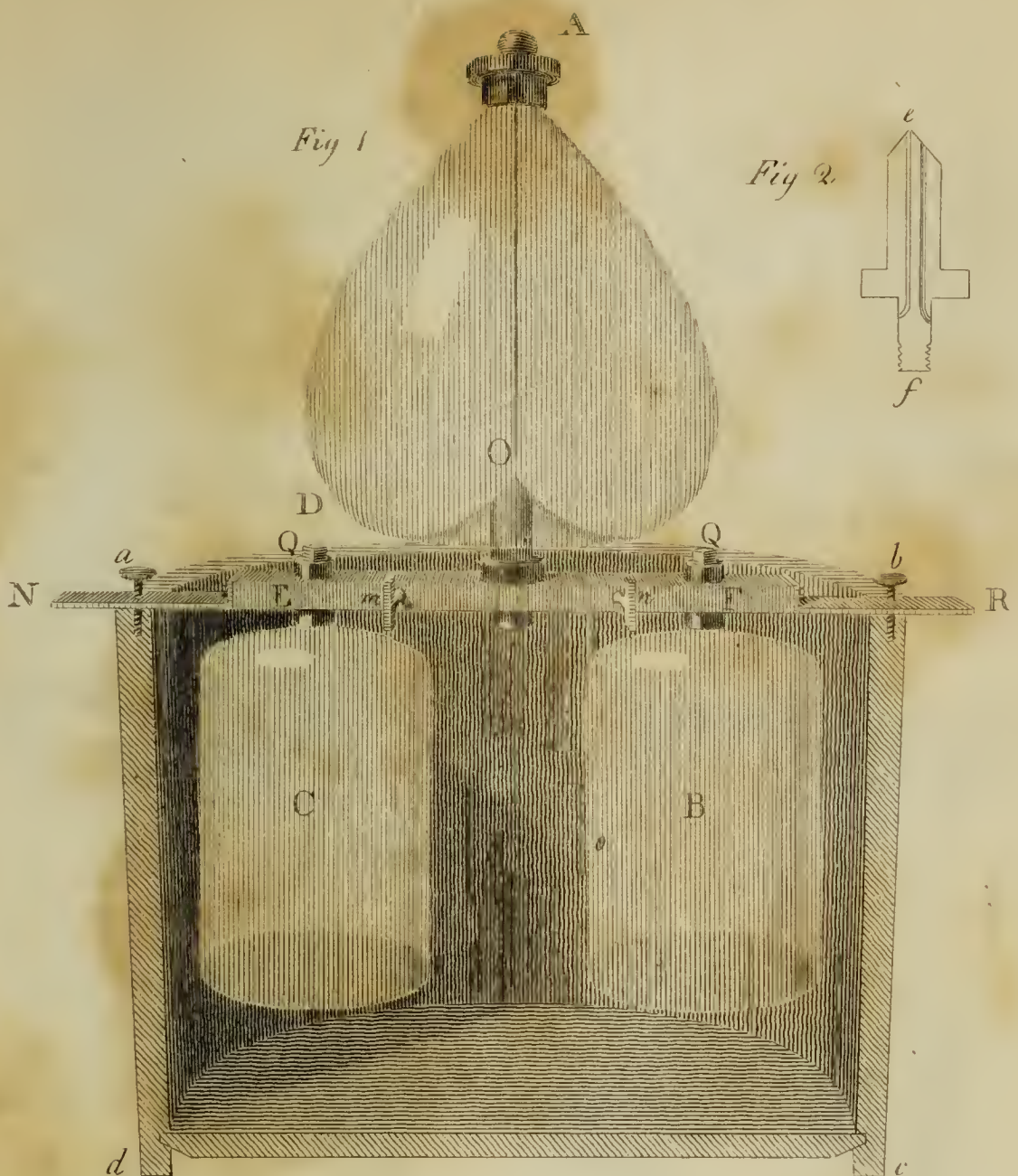

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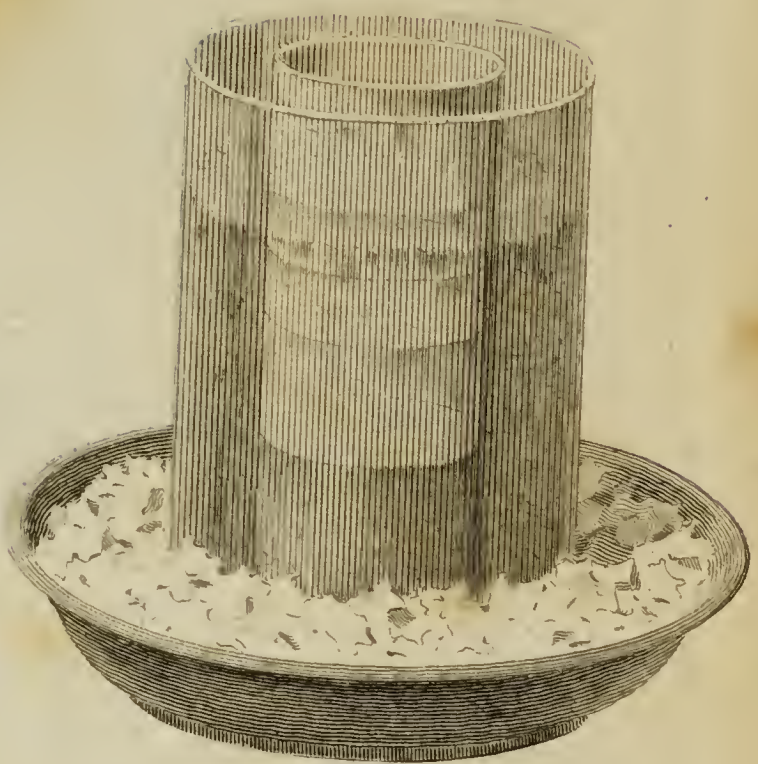

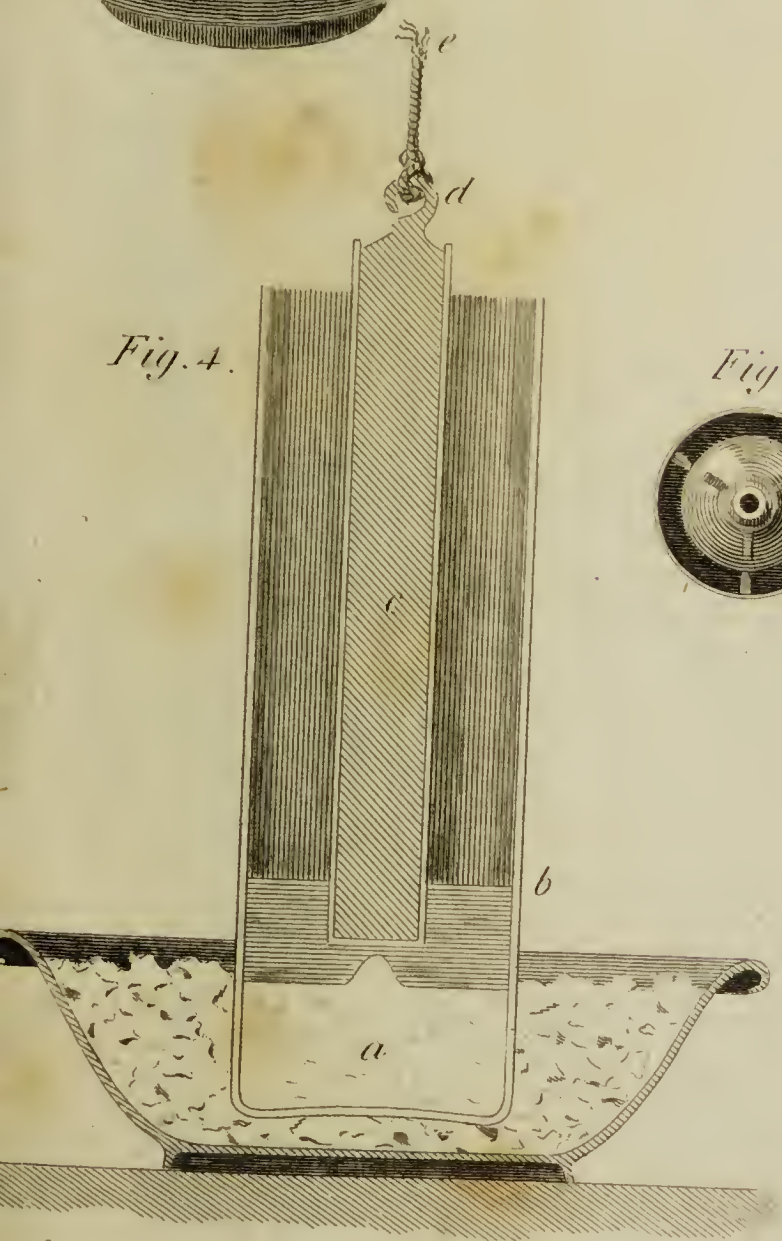

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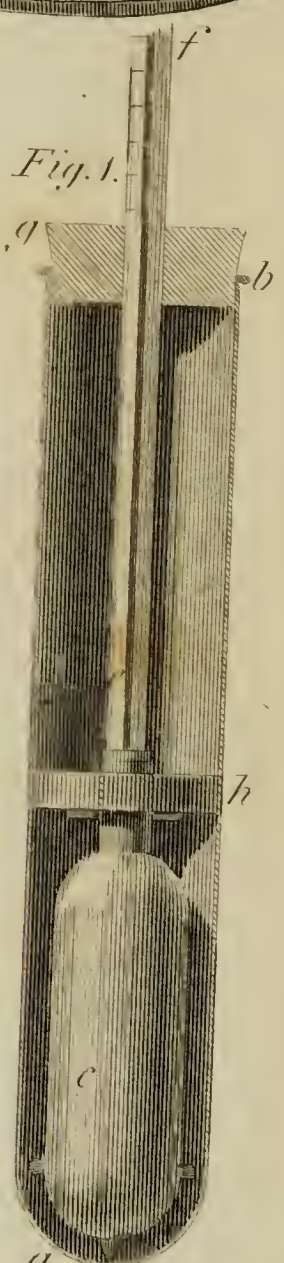

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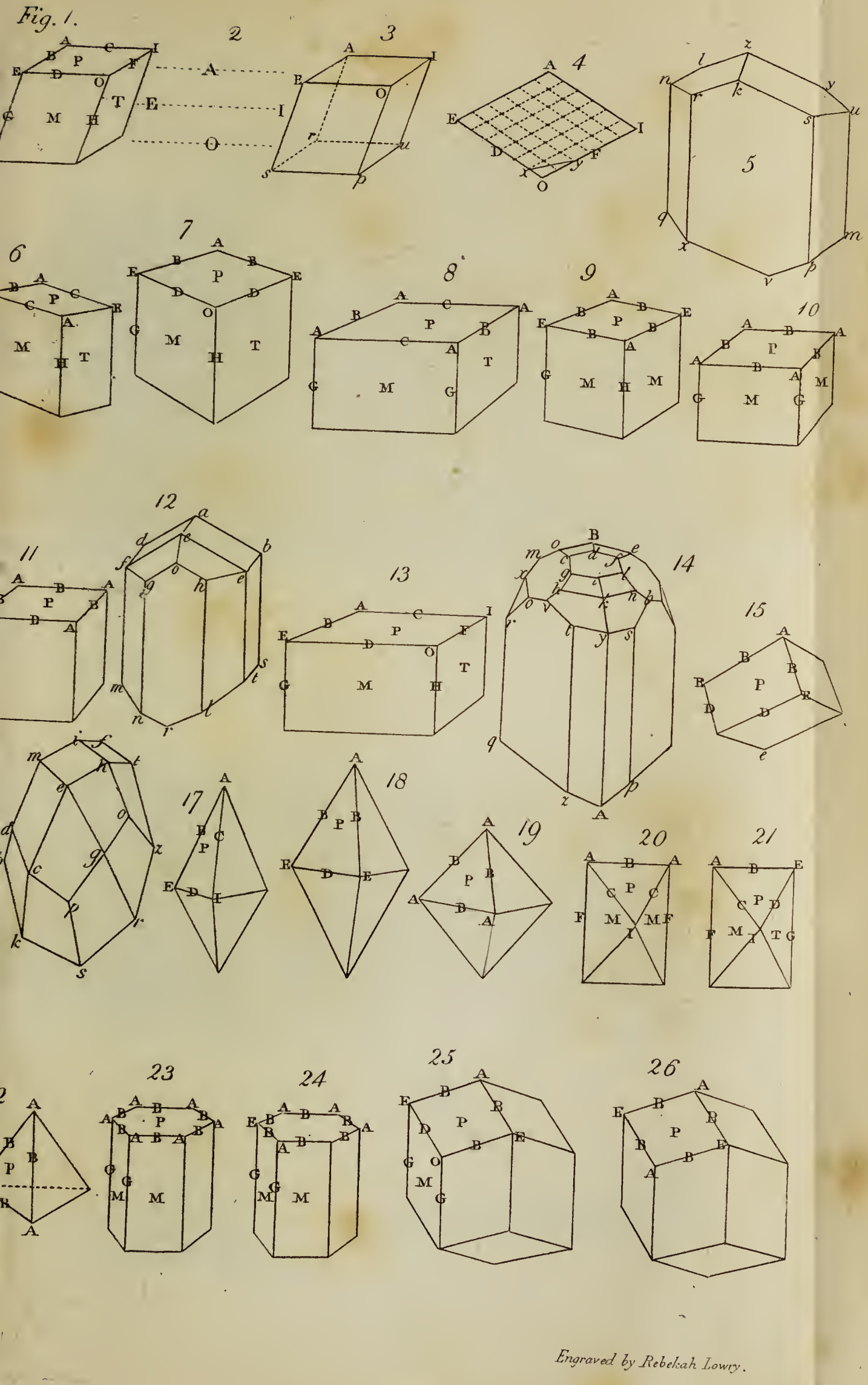

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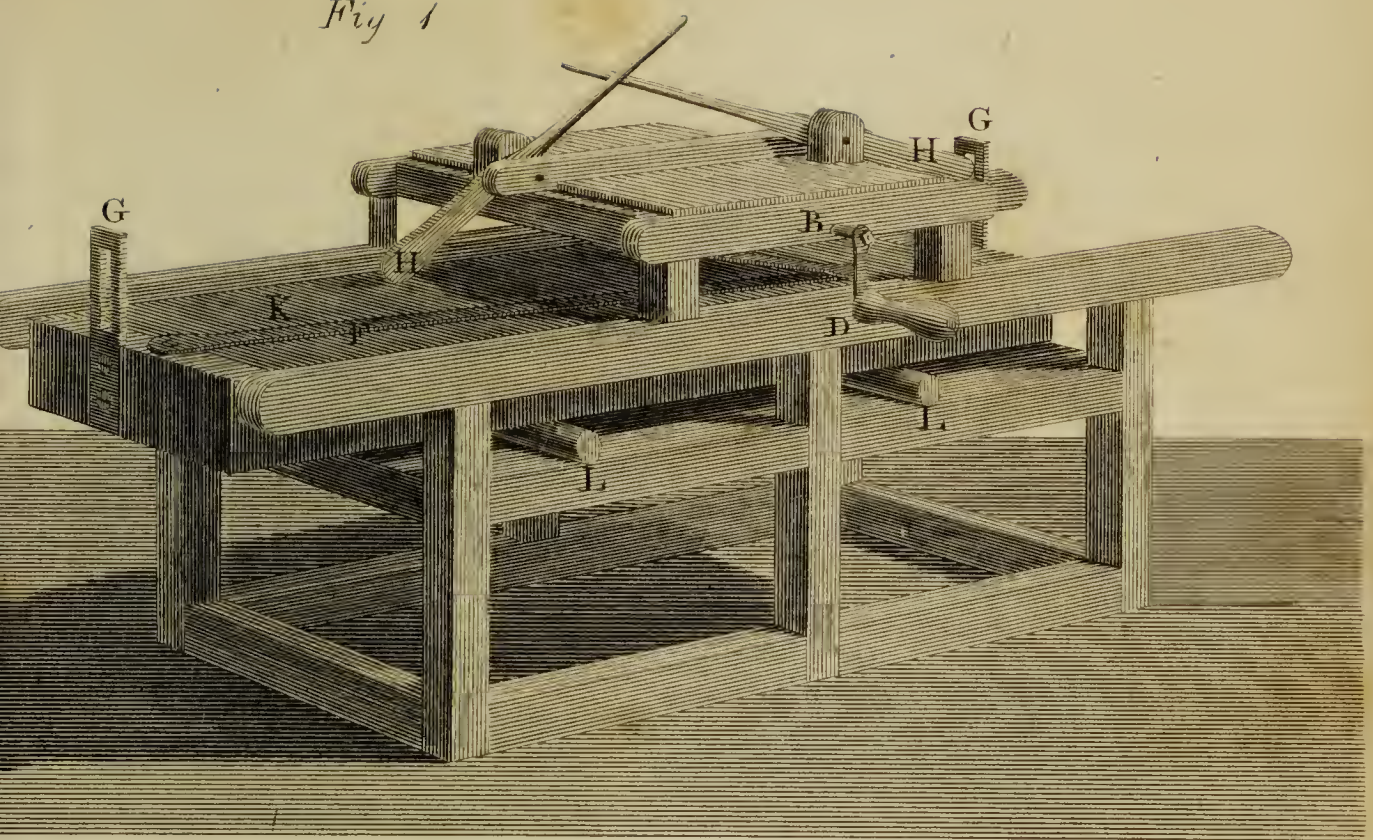

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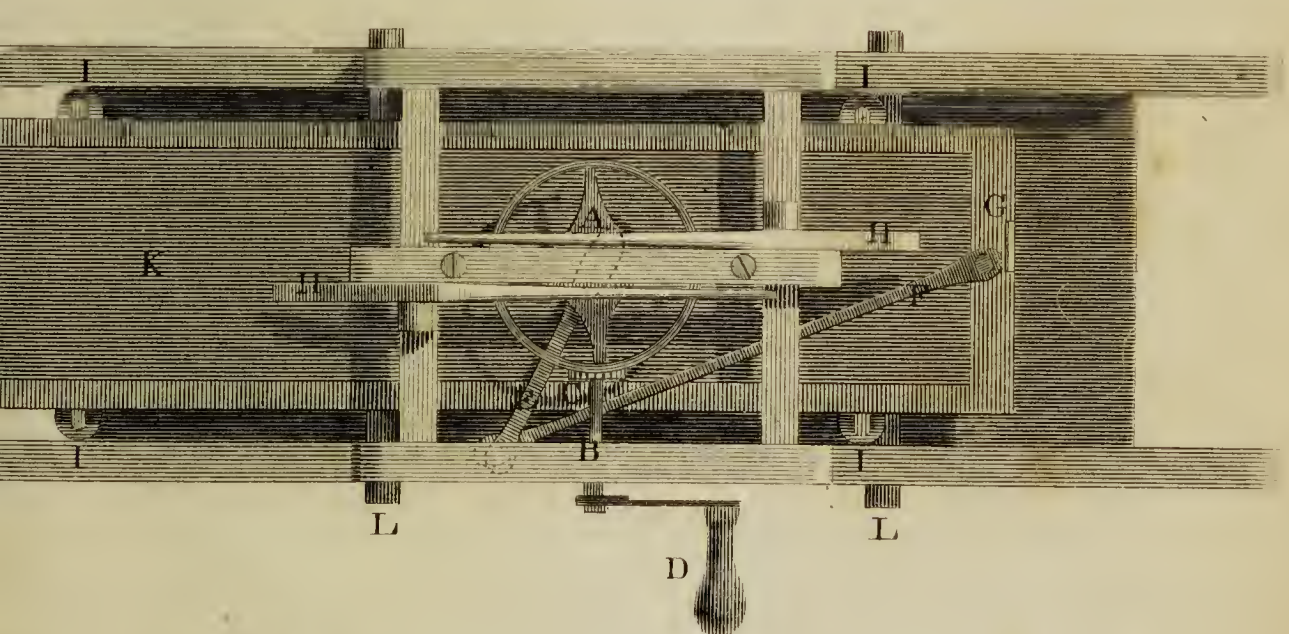

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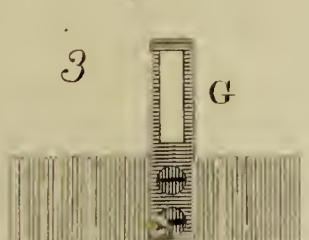

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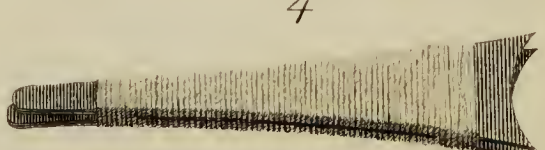

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